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THE PSYCHOLOGICAL REVIEW

THE PSYCHOLOGY OF 'ATTITUDES'

PART II

BY MUZAFER SHERIF AND HADLEY CANTRIL

Princeton University

SOCIAL FACTORS IN LABORATORY AND EVERYDAY LIFE SITUATIONS

Toward the end of Part I, which appeared in the November number of the REVIEW, we discussed the formation of frames of reference in individual situations. We proceed now to the consideration of attitudes in *social* situations, ranging from laboratory experiments involving social factors to complicated social conditions of everyday life. In doing so, we should stress again our conviction that in spite of the enormous variation in the content of attitudes, psychological principles of attitude formation are essentially the same, irrespective of what the attitude is concerned with.

The first stage of attitude formation —in the most complicated social situation as well as in a restricted laboratory experiment—is a perceptual stage. The individual must somehow come into contact with the stimulus situation before any attitude is established. In strict laboratory situations this stimulus situation is generally neutral and the frame of reference established lacks the vital affective and motivational properties so characteristic of social attitudes: in the laboratory experiment the stimuli may be a series of magnitudes, such as lines or weights or perceptual structures; in social life the stimuli may be a person, group, an expressed attitude or prejudice, a threat, or a value-judgment (norm) of any kind. Some of these stimuli in social life may be relatively neutral, may have only a mild affective property, *e.g.*, the color of a dress, the shape of a house, the characterization of a city, while some may involve intense, sometimes even traumatic, experiences, *e.g.*, a girl whom one loves at first sight, a scolding received by a child for cheating, an announcement over the radio that the Japanese have bombed Pearl Harbor.

In dealing with attitudes in social situations, then, we are dealing with stimulus situations rife with affective and motivational properties, with situations that frequently involve problems of the individual's status. It might be mentioned in passing that attitudes to situations involving affective properties seem to be more readily established (learned), just as Pavlov, Tolman and others showed that conditioning was more effective under motivational stress. Whether the perceptual experience involves direct contact with an object, person, or group or whether the perceptual experience involves the verbal transmission of a social norm by some short-cut value-judgment, makes no difference whatever as far as the basic psychological characteristics of attitude

formation are concerned. Both the soldier who has had direct experience with the enemy and his sister back home who has been subjected to value-judgments concerning the enemy can and often do develop various negative attitudes. It is therefore systematically useless to try to categorize attitudes according to their source or origin. This is, of course, not a denial of the specific properties of particular attitudes formed in specific situations.

Irrespective of the particular way in which an individual acquires an attitude in social life, the literature of social psychology is rife with data which support the formulations reached from our survey of general psychology: that perception and judgment are selective and occur within a referential framework, that frames or points of reference are inevitably established if an individual repeatedly faces the same stimulus situation, that these frames and points of reference are by no means always confined to consciously accepted instructions or imposed norms but can become established without the individual's realization of it, and that once established these frames and points of reference serve as anchorages for perception and judgment.

Experimental formation of a frame in group situations. This series of experiments was undertaken with the methodological conviction that the principles formulated and verified in general psychology are valid principles that operate in individuals participating in group action or collective behavior. Results produced by group interaction may be, and in fact are, different and may have all the earmarks of emergent products. Nevertheless, the psychological principles involved in the group situation are the same in operation. The emergence of new qualities is not a unique property of group action alone—perceptions of forms, relationships and melodies emerge as unique qualities not found in individual parts. This emergence of new qualities on different levels—individual as well as collective—has to be stressed constantly because of the sharp dichotomies drawn by certain schools of thought. For example, the dichotomy of experimental psychology vs. cultural psychology made by the romantic German school represented by Spranger (117) portrays experimental (individual) psychology as atomistic, static, and cultural psychology as dynamic and meaningful with total qualities. Writers of the Durkheim school of spiritualistic sociology, for example, Blondel (13), Halbwachs (59), and Durkheim (46) himself, argue that new qualities emerge only in collective situations, whereas findings in psychology substantiate the view of dialectical materialism that emergence takes place in all levels—physical, biological, psychological and socio-economic. In a recent study full of implications for further research in social psychology, Tresselt and Volkmann experimentally demonstrated the 'production of uniform opinion by non-social stimulation' (125). These investigators express a general psychological fact when they state that "the mechanism for judging social stimuli is the same as the mechanism for judging non-social stimuli" (p. 242).

With this methodological concern in mind, we can turn to the results of Sherif's group experiments on autokinesis. In the group situations each subject reported his judgments *aloud*, and naturally was heard by the other subjects in the group. He in turn heard their expressed judgments. No primacy effect was introduced into the situation, a special point was made in the wording of the instructions to the effect that subjects could report their judgments in any order in any presentation. A subject who gave his judgment first in a

particular presentation, might be the last to report in the following presentation. The building up of a frame in group situations is a temporal affair established in the course of the experimental period—the influence of expressed judgments is not restricted to judgments given at particular presentations.

Some groups faced the autokinetic situation *without any previous individual experience.* An equal number of groups faced it *after* the members had established their individual frames alone in individual experiments. The members of the groups who *first* took part directly in the group situation were therefore naive and neutral in relation to this unstructured, ambiguous situation.

The results are clear. When individuals as members of a group face the same unstable, unstructured situation without any previously established personal relationship among them, a scale (frame) and a standard (reference point) within that frame are established in the course of the experimental period.[10] When a member of the group faces the same situation subsequently *alone,* after the frame is established in the group situation, he experiences the situation in terms of the frame established in the group situation as a consequence of group interactions.

This result was verified by Asch and Wright in experiments performed in 1937. We shall give these results here in Asch's own preferred terminology.[11]

[10] Here we shall not digress to indicate some interesting results which revealed the differential, relative contributions of individual members. These points are important for the psychology of personality and will be considered in a forthcoming volume.

[11] This is a summary statement of investigations the full account of which will be reported in a forthcoming publication by Dr. S. Asch. The results reported here were personally communicated.

"The outstanding feature of the investigation of Sherif is the tendency of the subjects in the given experimental situation to reach mutual agreement. S. E. Asch and B. Wright undertook in a series of experiments to examine more closely the specific reasons for this tendency.

"Before proceeding with their experimental variations, they first repeated the main experiment of Sherif with four pairs of subjects. The arrangement of the experiment was as far as possible identical with that of Sherif. The main findings of Sherif were confirmed: a new norm (level of response) developed under the given conditions. However, the convergence proceeded along different paths in the different pairs of subjects. For example, some members exercised more influence on the direction than others. This finding is entirely consistent with the results of Sherif." (Variations of these experiments by Asch and Wright will be reported in their appropriate context later.)

More recently, in a series of studies dealing with 'some social factors in judgment,' Schonbar investigated modifications of judgments in 'situations of medium and high structure' respectively (110). From her results she concludes "our findings confirm and extend the conclusions arising from Sherif's work on the autokinetic phenomenon" (p. 129). That the convergence she found in cases of high structure is almost identical to the convergence found in cases of medium structure is puzzling, in view of the differentiated results obtained with varied degrees of structure and other factors by Asch, Lewis and Hertzman, Coffin, Chapman and Volkmann, Luchins, and Asch and Wright.

In Sherif's experiments with autokinesis, when individuals *first* establish their frames and standards in individual sessions, and are *then* brought into group situations, their judgments tend

to converge. But the convergence is not so close as when they first work in the group situation. When an individual comes into a group situation, with his own established frames and standards, there is a tendency to stick to them to some extent: individuals bring into the situation their own established frames and are no more naive in relation to the situation (113, p. 31, 41; 114, p. 104).

In one variation of these experiments, Asch and Wright gave contradictory instructions in the individual sessions, by prescribing varying ranges of movement. "Under these conditions of contradictory individual norms, no convergence developed in any of the pairs. Subsequent questioning revealed that fully one-half of the subjects did not realize that they were in contradiction with their partners. They interpreted the addition of the integer 3 (or the dropping of the integer 3) as introduced for the sake of identifying each of the subjects in the dark room. The remaining subjects interpreted the existing difference in terms of such factors as differences of position with regard to the light, differences in eyesight, etc." This precisely means that even following individual sessions, with prescribed contradictory instructions and contradictory standards and norms, half of the subjects converged. The addition or dropping of the integer '3' (which the subjects thought was introduced as an experimental device) accounts for statistical but not for psychological nonconvergence as explained by the subjects who "did not realize that they were in contradiction with their partners."

In a more decisive variation aimed at the complete destruction of convergence, Asch and Wright performed the following experiment. In Asch's words, "The aim of the following experiment was to alter in a decisive way the cognitive character of the situation. The subjects were informed in the individual session concerning the subjective nature of the autokinetic effect (each subject was paired in the present variation with a 'planted' subject whose estimates differed considerably and consistently from the experimental subject's). There were ten subjects in this experiment. Under these conditions, five subjects showed no convergence effect whatsoever; the other five did depart significantly from their previous judgments and in the direction of the 'planted' subject. To explain the two forms of reaction, the subjects were questioned at the conclusion of the investigation. According to the present results, it seems that the subjects who showed the convergence effect continued to think of the autokinetic phenomenon as objective and *forgot the preceding instructions*. Nevertheless, the change found in the first half of the group conclusively demonstrates how effective the alteration of the cognitive character of the situation can be" (see ftn. 11, italics ours). In spite of the fact that the subjects were told *beforehand* that the light was not moving at all and in spite of their individually established norms before coming into the group situation, it is surprising that there was any degree of convergence at all. We do not know if the fifty per cent convergence attributed by Asch to forgetting is in line with current forgetting curves. The lapse of time between the individual and group sessions cannot be longer than a few days.

Nevertheless the above variations which introduced into the situation various degrees of familiarity or objective knowledge of the situation account for the variations in the results obtained. They are in harmony with Sherif's results which showed that once individual frames are established, convergence or agreement in subsequent

group situations is decidedly affected by them. Likewise, the findings in these variations are in harmony with the findings of Chapman and Volkmann that previous familiarity with a situation prevents shifts in the direction of experimentally introduced standards or norms. Likewise, they are in harmony with the finding of Luchins that social influence varies, among other factors, with variations in structural clarity. And all are in harmony with the formulation reached in the previous section that the influence of internal and social factors increases directly with the increase of the unstructuredness or ambiguity of the situation, and decreases with the degree of the structuredness or unambiguity of frames and standards already established by an individual.

Experimental inculcation of a frame. As we stressed above, frames are formed in the individual in daily life corresponding to objects and norms around him which are compelling in their objective structure. Quite frequently attitudes are formed as a direct consequence of short-cut verdicts or value-judgments of grown-ups, teachers and others around us. These value-judgments may or may not be imbedded in the truth of actual relationships. We are concerned here with the psychological process involved in their acquisition. The formation of such a frame in a social situation has been experimentally demonstrated. The experiment was carried out by Sherif in the spring of 1936 at Columbia University again utilizing the autokinetic technique (115).

The specific problem in the experiment was to see whether 'naive' subjects could be experimentally inculcated with varying ranges of prescribed frames and standards. Preliminary experimentation had shown that a partner in this situation (in this case a partner with considerable prestige) could influence the judgments of the other subject—raise and lower them in his direction. When the 'naive' subject was told exactly what had happened she became very disturbed, an indication that an individual can get emotionally upset by being so fooled (115, p. 92).

In the main experiment there were seven groups of two members each. In each group, one member coöperated with the experimenter by deliberately distributing his judgments around the range (scale) and a standard point within that range prescribed beforehand by the experimenter. The second member (designated as the 'naive' subject) was totally unfamiliar with the situation. In each case the naive subject was not acquainted either with the experimenter or the coöperating subject. To be sure that the conformity was to the prescribed range and standard, a different range and a different standard were prescribed for each group. In order *to avoid the factor of primacy*, the coöperating subject was instructed to let the 'naive' subject express his judgment first at least half the time. The first session was the group session with both the coöperating and 'naive' subjects participating. In the second session the 'naive' subject was *alone*.

The results indicate considerable convergence to the prescribed range (scale) and standard in the judgments of the 'naive' subjects. The varying convergence in different subjects clearly indicates personality differences. The convergence of the judgments in the 'alone' session, which took place the day following the group session, was, in general, even greater than that obtained in the group sessions. Introspections reveal that the subjects became conscious of the scale and standard formed during the course of the experiments: Five subjects out of seven reported in their written introspections at the end of the experiments that they were not influenced

by the judgments of their partner. This clearly indicates that a social influence which continues even when the individual is alone need not be consciously experienced. Schonbar has recently obtained results which lead to the same conclusion (110).

It is a significant fact to find convergence greater, in the majority of cases, when the individual is acting *alone*. It is not a rare occurrence in everyday life to react negatively or hesitatingly on some topic raised by some person while in his presence, but to respond positively when he is no longer in the situation. To yield easily is not a pleasant 'ego' experience.

This experiment embodies in itself the rudiments of the psychology of attitude formation: a scale (or frame) is *formed* under the influence of the verdicts of another individual *in relation* to a definite, experimentally controlled stimulus situation. The scale (frame) is carried from one day to another, has to some extent an *enduring* quality which provides an individual with a state of readiness by means of which he reacts in a *characteristic* way to the same stimulus situation. Here we have the main earmarks of an attitude. We must admit at once that this formation lacks the *affective* quality of a real attitude. But we do not believe this at all invalidates our conceptualization. The investigations of Beebe-Center (9) and associates Cohen (41) and Hunt and Volkmann (67) all indicate that the mechanisms of affective judgments are not essentially different from those of other judgments. Besides, as we shall see there is further evidence to support the general scheme so far presented.

Social factors in more concrete and everyday life situations.—The conceptual scheme offered here towards the psychology of attitudes may appear to be an 'artifact' based only on the results of laboratory experiments which have no counterparts in real life situations. So we shall turn now to some examples of more everyday life situations which seem to us to confirm the reality of our formulations. We cite here only a few of the many studies—ranging from relatively simple demonstrations to complex reactions of daily life—that confirm the conclusions reached in the more strictly laboratory settings.[12]

In his series of experiments (one of which has already been referred to), Coffin investigated the psychology of suggestion as it was related both to attitudinal structure and to stimulus situations (40). In his first experiment Coffin studied the relationship between an individual's attitude and the type of propaganda to which he was particularly susceptible. He found (in the winter of 1939 and 1940) significant correlations between pro-Allied attitudes on the one hand and the acceptance of specially prepared pro-Allied propaganda on the other hand. Conversely those with pro-German attitudes accepted pro-German propaganda to a significant degree. In a second experiment Coffin used the Rorschach inkblots as stimuli with little structuration. After his subjects had rank-ordered a list of ten occupations according to their social standing, they were divided into

[12] We might incidentally call attention here to the fact that most experiments on humans in the psychological laboratory are social situations at least to the extent that they involve verbal material in the instructions given to subjects. The variety of possible meanings of stimulus situations persistently gives trouble since meanings so readily intrude themselves (50, 109, 141). It is well known that a subject's 'set' or 'attitude' can profoundly affect his reactions in psychological experiments. It will also be remembered that Titchener, in trying to create the 'proper' set, advocated the use of 'trained observers.' From our present vantage point these results obtained only from observers trained to rule out objective references seem highly artificial.

two groups; each group was told the characteristic response given to the Rorschach inkblots by professional men, by business men, by skilled laborers and by those on WPA, but the various characteristics were attributed to different occupations in the two experimental groups. The results clearly showed that when these ambiguous stimuli were used, the subjects were highly influenced by the suggestions given which served as anchorages. They actively structured the imaginative situation according to the 'characteristic reaction' to the blots of occupational groups they believed had high social standing. In a third experiment, testing the relationship between suggestibility and the difficulty of problem solution, Coffin found that when mathematical problems were arranged in order of difficulty with marginal (and usually false) hints (anchorages) beside each problem as to what procedure might be used in its solution, respondents more highly trained in mathematics followed the marginal suggestions less: those who knew the most mathematics accepted less than half as many of the suggestions as those who knew least mathematics. Those persons, therefore, least able to make successful checks for themselves were most susceptible to an imposed instruction.

We have cited Coffin's experiments here at some length, since they provide unequivocal evidence of two important conclusions: first, that if a suggested stimulus is related to and consistent with an established frame of reference, it is likely to be accepted; and, second, if a stimulus situation is ambiguous or relatively meaningless because of its difficulty, then a frame of reference verbally imposed by the experimenter is readily accepted as the basis for judgment.

A study by Ruth and Eugene Hartley concerned with poorly defined situations is particularly significant since it shows how individuals establish 'individually characteristic ranges and reference points' by means of which they judge a relatively unstructured social stimulus (59a). The Hartleys had students rate a series of pictures of completely unknown men. Among other ratings, the subjects were asked to estimate each man pictured on his 'general ability' and his 'likeliness to succeed.' They found a constancy of ranges and central tendencies in the judgments and conclude that "In making primary evaluative social responses to people, when little information is available, individuals tend to manifest characteristic ranges and central tendencies with reference to which judgments are made."

The research of McGarvey "was undertaken in order to determine whether relations similar to those which have been found to hold with the psychophysical materials would appear also in judgments of verbal material along value-dimensions" (93, p. 26). In her first experiment, McGarvey had subjects rate the social prestige attached to a number of occupations. After they had rated all occupations on a graphic scale, in further experimental sessions the top or the bottom category of the scale was set by an 'anchoring value'—a given occupation was used to define one extreme or the other. In the second experiment the items judged were short descriptions of various types of social behavior to be evaluated in terms of their desirability. Again 'anchoring stimuli' were introduced at either end of the scale. On the basis of these experiments McGarvey concluded that "the effect of the anchoring value was that of bringing about an extension of the absolute scale upward or downward in the direction of the anchoring value. The extension of the absolute scale involves not only a displacement of the scale with reference to the range of

values represented by the stimulus-series, but also a widening of the categories of response—a finding in complete agreement with the results obtained with psychophysical material" (93, p. 78). In other words, judgment of a given stimulus is found to be determined by the frame of reference within which that judgment occurred and the introduction of new anchorages changed the dimensions of reference frames. This precisely parallels on a social level the results obtained by Rogers cited in Part I.

The close relationship between memory and an individual's frame of reference was exhaustively shown by Bartlett who concludes that "remembering is schematically determined" (7, p. 312), "an imaginative reconstruction or construction, built out of the relation of our attitude toward a whole active mass of organized past reactions or experience, and to a little outstanding detail which commonly appears in image or in language form" (7, p. 213). "The circumstances that arouse memory orientation, whether they occur in the laboratory or in everyday life, always set up an attitude that is primarily toward a particular 'schematic' organization" (7, p. 312). What a person remembers, as well as what a person observed of a given stimulus situation, was, he found, clearly influenced by the particular social origin of the individual's attitude. He showed how these attitudes—and consequently recall and observation—varied according to the cultural background of the individual, how they differed among members of various groups within a given social system, and how they differed within the same individual when he was or was not in the actual social presence of other members of his group. Various subsequent studies have shown that material consistent with a person's attitude is much more likely to be remembered than material not consistent. Seeleman, in stating the problem of her experimental study of memory, says that "these differences in standards may be expressed as differences in subjective norms or frames of reference" (111, p. 7). She analyzed the influence of the attitude toward the Negro on the remembrance of pictures of whites and Negroes to which were attached, in one experiment, favorable or unfavorable phrases supposedly describing the individual shown. She found that persons with extremely unfavorable attitudes toward the Negro recognized fewer individual differences between the Negroes shown in the pictures, recognized correctly fewer Negro than white pictures, assigned to Negro pictures more unfavorable phrases and remembered these unfavorable phrases attached to Negro pictures more accurately than persons favorable to Negroes. The evidence from these experiments is, then, that the attitude toward the Negro affects perception as well as memory. Zillig found experimentally that women tend to remember more items favorable to women, while men tend to remember more items favorable to men (142). Watson and Hartman demonstrated that a person's attitude toward atheism clearly determined what he would remember of material concerning atheism and theism (133). Edwards showed that the political attitudes of his subjects significantly determined their recognition of items contained in a speech they had previously heard about the New Deal (47). In another study Edwards demonstrated the extent to which a person's attitude stimulates him to rationalize his answers to factual statements with which he disagrees (48). Levine and Murphy have shown experimentally that both the learning and the forgetting of passages that are favorable or unfavorable to the Soviet Union are significantly af-

fected by a person's attitude toward communism (88a).

The psychology of testimony presents many vivid illustrations of the influence of attitude on observation, judgment, and memory. Stern's early experiments on *Aussage* showed the suggestive effect of leading questions and demonstrated that the effect of such suggestions often became 'stabilized' to influence later or allied judgments. He also demonstrated how persons tended to describe occurrences from the point of view of what was to them the 'customary' or 'usual' way for such occurrences to take place. The attitude a witness has toward a dispute and a witness' own attitudes toward situations were apt to have considerable influence on the testimony of the witness, even though he might be a highly-educated, cautious individual (120, 121).

In an unpublished study by Lazarsfeld and associates of the reaction of individuals of various nationality and racial backgrounds to the same motion picture, the effect of attitudes in determining what will or will not be observed and remembered and what emotional reactions will be aroused in different people by the same stimulus situation is clearly demonstrated. The film was a short British war picture, *Naples is a Battleground,* shown to the experimental groups in the early summer of 1944. Native white Americans tended to see in the film an example of American armed might and remembered particularly the showing of General Mark Clark; Negroes in the audience paid special attention to the Negro troops pictured; while most marked of all were the reactions of first generation Italians who were impressed, saddened, or horrified by the ruins and extent of devastation. The experimenters observed that "There were certain aspects of this film that stood out for some but not for others because of their differing mental sets as they watched it" (87).

In a study of rumors current during 1942, Knapp found that "rumors become harmonized with the cultural traditions of the group in which they circulate" (79, p. 30). Rumors are given particular twists as they penetrate groups, where characteristic norms are found, such as a high degree of anti-semitism, anti-Negro and anti-British sentiment. He noted, for example, that in the Italian section of Boston rumors of enemy submarines outside Boston Harbor were similar to rumors in other areas of the city except that people in the Italian section believed the submarines were Italian.

Numerous experiments on the level of aspiration have shown how the same performance can be regarded either as a success or a failure depending on the frame of reference in which the performance occurs, as, for example, the study of Gould and Lewis where an individual defined success differently when comparing himself to the supposed performance of college professors than when comparing himself with the supposed performance of WPA workers (57). Similar results were obtained by Hertzman and Festinger (62). Frank has demonstrated that when a level of aspiration is set within a frame of reference clearly involving the ego it is likely to differ from non-ego involving levels of aspiration by being consistently higher than the level of past performance (52, 53). Gould has pointed out the importance of the acceptance of various cultural norms as determinants of level of aspiration and has indicated that the meaning of differences between level of expectation and level of performance for any individual under any given conditions can only be understood with reference to his genetic development (56). In the work of Chapman and Volkmann previously mentioned the

setting of a level of aspiration was studied under two sets of conditions (39). In one experiment involving familiarity with the authors of various literary passages, each of the different experimental groups was given in advance of their direct acquaintance with the task involved the score supposedly made by another group. The score attributed to each group was the same but the groups varied in their prestige: authors and literary critics, students similar to the subjects, and WPA workers. Results here show a clear-cut tendency for those comparing themselves to a superior group to lower their aspiration level, those comparing themselves to an inferior group to raise their aspiration level. In their second experiment, involving a test of mental ability, all subjects were given a test in two sessions before any attempt was made to change their aspiration level by comparing their performance to inferior or superior groups. Under these conditions, no significant change in the level of aspiration was produced; "the subjects own previous scores provided the most effective anchoring" (p. 235). In the conditions of the first experiment, the stimulus situation is unstructured and an imposed norm is accepted as a frame of reference; in the conditions of the second experiment, the stimulus situation has become structured through experience and imposed norms are ineffectual.

Asch, Block and Hertzman in 1938 showed that judgments concerning the characteristics of different professional groups tend to be considerably modified when an evaluation from an authoritative source is introduced (3). A close relationship was found between judgments on different characteristics of various professions indicating that most of the judgments seemed to derive from underlying attitudes. They conclude that "the judgments of a single situation are related to each other by a person in accordance with an underlying attitude of acceptance or rejection" (p. 245), that "a standard having an authoritative source tends to alter an individual's judgment in its direction" (p. 249), that relationship in judgment becomes most clearly established "for situations which are not well defined objectively" (p. 251) and that "the observer, in the absence of objective criteria, and in the face of the necessity of reaching some conclusions, proceeds to arrange a scale of preference in terms of some generally favorable or unfavorable impression" (p. 229).

Kay's study of the relationship between personal frames of reference and social judgments indicates that an individual's evaluation of various occupations is considerably more affected by accepted social norms relating to those occupations than by an individual's own preference for an occupation or his experience with it (75). In other words, the social norms concerning the value and characteristics of common occupations are rather uncritically taken over by individuals as personal frames of reference by means of which specific judgments are made. Analyzing her data further according to the source of information concerning the various occupations rated, Kay found that for a third of the occupations 'cultural' sources were mentioned, most frequently while the 'personal' sources uncovered in the interviews were interpreted or reacted to on the basis of 'personal frames of reference' and 'in the light of existing social norms' rather than on the basis of the 'objective quality of the experiences themselves' (76, p. 363). This interpretation confirms Davis' study on the attitudes of children in Soviet Russia with its finding that Russian children rated laboring people high, lawyers and bankers low (42). In their examination of the basis of prestige judg-

ments of various occupational groups, Osgood and Stagner demonstrated that the "decisions about characteristics of occupational stereotypes tend to conform closely to a framework, which is based on the relative prestige of occupations" (106, p. 287). They conclude that "the mere presentation of a set of occupational stereotypes for a series of judgments caused our subjects spontaneously to establish a prestige framework which then determined in a highly reliable manner judgment on the specific traits listed" (p. 289).

Asch found that an individual's judgment of relatively ill-defined and unclear situations could be changed when the imputed judgment of congenial groups was introduced as a reference (4). On the other hand, subjects tended to reject the judgment of antagonistic groups. Here again we have evidence that an individual tends to accept an imposed norm as his own frame of reference for judging a situation when that situation is itself unclear or when he has no preëxisting, sure, or ego-involved frame of reference of his own. Cantril's analysis of radio listeners who followed a particular commentator indicated that the chief function of a news commentator is to provide frames of reference by means of which listeners can judge the plethora of events going on around them (27). The less people know about objective conditions, the more they depend on a commentator to tell them what these events mean and to help them select items to read in their newspapers.

One of the most penetrating studies of the development of prevalent attitudes in our social system is that of Horowitz on the genesis of attitudes toward the Negro (66). He demonstrated unequivocally that this attitude is imposed bodily and uncritically without any basis in experience or knowledge. He concludes "that attitudes toward Negroes are now chiefly determined not by contact with Negroes, but by contact with the prevalent attitude toward Negroes" (p. 35). Horowitz developed a series of ingenious tests to measure attitudes toward the Negro objectively. These tests were administered to children and adolescents in a variety of social groups—children in the rural and urban South, all white groups in New York City, mixed groups in New York City, and a group of New York communist children. His tests showed that children in New York City were just as prejudiced as the children in the South, that children in mixed schools were as prejudiced as those in all white schools, that contact with popular Negro children had no effect on attitudes, and that the only group of children tested who had no prejudice against the Negro were children of communist parents—people devoid of racial prejudices which they would pass on to their children. Furthermore, Horowitz found that Negro boys in mixed schools tended to accept some of the racial attitudes of the white majority of their group.[13] His findings confirm the earlier conclusions of Lasker's well-known study that race attitudes in children are due chiefly to 'the absorption of adult attitudes' (86, p. 371), and that contacts between children of different races are almost invariably influenced by the 'adult-made environment' (p. 371).

In a significant study concerned with the development of stereotypes toward the Negro, Blake and Dennis (12) had students from grades 4 through 11 in a southern school compare whites and Negroes on 60 characteristics. Their results showed that there was less agreement among the younger children who

[13] This is reminiscent of the fact that in the days of slavery in this country there was a sizeable number of freed Negro slaves who, when they acquired land of their own, retained Negro slaves to work it (54).

had a 'relatively undifferentiated' attitude unfavorable to the Negro. They conclude that "the young white child acquires first of all a generally unfavorable attitude toward the Negro, which makes him unwilling to attribute to the Negro any 'good' traits. With increased age and experience, the child gradually learns to apply the adult stereotypes, a few of which are complimentary" (p. 531). The extent to which the accepted attitude toward the Negro affects judgment and goes against all evidence has recently been illustrated by a public opinion survey on a nationwide sample of the adult white population. A majority of those with opinions believe that Negroes are not as intelligent as white people and cannot learn as well if they are given the same education. And a third of our white population believes that Negro blood somehow differs from white blood, a third of the people believes it is the same, and a third say they don't know (104). Murphy and Likert concluded from their study of attitudes toward minority groups that "the individual usually acquires his prejudices against a minority group not primarily from contact with this minority group but chiefly from contact with the prevailing attitude toward this minority group" (98, p. 136).

The bodily acceptance of attitudes is further confirmed by studies which have shown the ease with which people characterize various nationality groups, irrespective of their lack of knowledge about or experience with these groups. Katz and Braly found that the preferential ranking of students for various nationality and racial groups closely followed a weighted ranking of the judgment of a comparable group of students on the stereotype of these same groups (73). Public opinion surveys have shown that less than 10 per cent of the population feels unable to select from a list of adjectives those that best describe various nationality groups and that people in our culture have generally similar stereotypes concerning the characteristics of major nationality and racial populations (18). The extent to which stereotypes influence judgment has been clearly demonstrated experimentally. Sherif, for example, showed that college students both in the United States and in Turkey were significantly affected in their rating of a literary passage by the name of the author attributed to that passage (113). The passages used in the United States were all by the same author, those used in Turkey by another. The judgments of these passages, then, were made largely in terms of established values. These results are similar to earlier demonstrations of everyday stereotypes shown by Zillig (142) among others, and were confirmed later on still another type of material (24).

It is obvious that the whole psychology of fads and fashions is to be explained largely in terms of accepted norms and values that provide standards of judgment for style, correctness, and certain criteria for status, beauty, significance, and the like (126). In a brilliant note 'concerning scientific progress' at the end of his *History of Sensation and Perception*, Boring shows that even scientists, including psychologists, tend to conform to the *Zeitgeist*, are retarded by habits of thought currently fashionable or by 'laboratory atmosphere' (16). It should be pointed out in this connection, however, that atmospheres of psychological laboratories, as well as more common fads and fashions, sooner or later change with new factors imposed by objective conditions or by the accumulation of evidence. The dresses women have for everyday wear in the western world can no longer be styled without reference to the demands of the machine age in

which they live, the Titchenerian atmosphere at Cornell could not remain unaffected by the facts of Würzburg. As facts pile up in psychology or any other science, laboratory atmospheres and 'schools of thought' become increasingly tenuous.

Newcomb measured the change of attitudes of students in a small college community where non-conservative attitudes were considered to be more 'proper' and to carry more prestige than conservative attitudes (103). He found that this 'community frame of reference' (p. 151) significantly influenced the attitudes of students in the liberal direction during their four years, irrespective of the courses studied in college. Newcomb describes this shift of attitude as general rather than as a shift of a series of specific attitudes toward specific issues. When events or new proposals were reacted to, the more advanced students more consistently reacted to them in a liberal way.

In his study of the psychology of status, Hyman has investigated the rôle of attitude in a complicated social setting (69). After learning from an intensive interview of each of his subjects something of the meaning, genesis, criteria of and satisfaction with status, Hyman constructed scales to measure subjective status along several dimensions: general status, economic, intellectual, cultural, social, and physical attractiveness. Subjects were also asked to indicate their subjective status with reference to different groups: the total adult population in the United States, friends and acquaintances, and their occupational group. He found that "within each status dimension an individual's judgment of his status shifts when reference groups are changed" (p. 49). Among other results reported, the following are particularly significant for our purposes here: individuals strive for status with respect to those accomplishments or characteristics which they most highly value; when an individual's status is approximately similar to the status of the group he is using as a basis for comparison, then he shows no particular concern for his own status, no great drive to achieve a higher status; persons who regard the difference between their own status and a reference group as being determined by a social system they disapprove of also show little dissatisfaction with their own status. Hyman's study clearly indicates that an understanding of the psychology of status is possible only if we have a precise knowledge of the frame of reference or anchorage by means of which an individual judges his status.

The way in which an individual's own present income provides a reference point for judging financial needs and aspiration has been demonstrated by Centers and Cantril (36). In a nationwide survey concerned with the relationship of present income to satisfaction and wants, they found that among those people dissatisfied with their present income, the larger this income is, the more additional money is wanted. An analysis by Fried and Cantril of judgments made by persons in different income groups of the income tax people in various income brackets should pay shows that those in the low income groups have such inadequate standards for judging the incomes of people in high income brackets that they tend to find such judgments difficult or impossible to make. Furthermore, when judgments are made by persons in the lower income groups of the tax which those in the highest brackets should pay, the figure given is significantly less than the tax upper income groups think should be paid by the rich, illustrating again the way in which judgment is anchored in individual frames of reference (55).

Cantril's study on identification with social and economic class has shown

that *in 1941* almost nine-tenths of the American people identify themselves with the middle class (32). Even 70 per cent of those who feel they are members of the lower income group still call themselves members of the middle social group. There is a strict correspondence between the two class identifications—social and economic—among only 54 per cent of a representative sample of the population. There is, then, a definite tendency for individuals to regard their social class within a frame of reference provided by the norms of their social system and frequently unsupported by the income necessary to solidify their own positions objectively in the social level they accept as their own. The greatest disparity between income and social identification is found among the low income group in 1941. The same study also indicated that those higher in the social class identification tended to base the identification less on strictly economic criteria. Their subjective social status was more affected by norms commonly accepted as high in the social scale such as family background, education, professional accomplishments, and the like. And the evidence indicates that there is considerably more ego-involvement in maintaining a relatively high social identification than a high economic identification (131).

Kornhauser's investigations of the class attitudes of various socio-economic groups have shown that in our American social system 'logically opposed interests' do not by any means lead to uniformly opposed class attitudes (83, 84). Kornhauser did find, however, that those persons within each economic group who were most dissatisfied in terms of their present status and opportunity did accept less frequently than others the traditional status quo as measured by attitudes concerning labor, government control, etc. And although the attitudes of income and of occupational groups were not diametrically opposed, still significant differences appeared between the attitudes of these groups on a number of social and political issues. In other words, the objective conditions imposing themselves did not fit currently accepted economic and social norms. Particularly significant for our purposes are Kornhauser's reports of the instances where no significant differences are found between the attitudes of different economic groups. These attitudes—accepted rather uniformly—were those which reflected the traditional American middle class ideology concerning individual opportunity either for the person himself or for his children.

These exploratory investigations of the psychology of class in contemporary America show that although an individual's attitude concerning the political-economic organization of the social system is affected by his circumstances and the norms of his economic group, the fact that the characteristic middle class American ideology is relatively uncritically superimposed on and accepted by individuals in all economic groups, results in the inconsistency of attitude so frequently pointed out, in the difference between attitudes people 'logically' should have and the attitudes they actually do have.

That the attitudes toward labels describing different political systems are by and large uncritically accepted without any real knowledge of the principles or implications of those political systems has been established in a number of different studies. Stagner demonstrated, for example, that people will have a distinctly unfavorable attitude toward fascism but at the same time will accept certain fascist doctrines (118). Menefee comes to the same conclusion in his studies which reveal that if the statement of a political or economic

principle is labelled as 'fascist' it will call forth a much more negative reaction than it will without the label (95). Edwards concludes from his investigations that "some college students have a far greater degree of sympathy for certain fascist principles than might be expected from their otherwise antagonistic reaction to the fascist label" (49, p. 580). Significantly enough, Edwards found that those persons who labelled themselves politically as 'independents' were considerably more critical of statements of fascist principles than those who called themselves Democrats or Republicans or, in other words, those who accepted uncritically the political labels of their own social system. Katz and Cantril found that the attitude toward fascism or communism bore little relationship to the knowledge of fascism or communism among college students in 1939 (74). Their study also shows that, although there was an overwhelming rejection of the terms 'fascism' and 'communism,' the majority of students felt at the time (before World War II had brought the implications of fascism into clearer focus) that fascism was a good thing for Germany and Italy but would be a bad thing for Russia and certainly for the United States, whereas communism was a good thing for Russia but would be a bad thing for Germany and Italy and especially for the United States. All of these studies show, then, that broad symbols tend to be accepted without objective reference.

Analyses of political attitudes have indicated clearly that for the vast majority of modern Americans the way in which they vote is determined chiefly by the political attitude they have accepted rather than by any analysis of different party platforms or candidates. Hartmann has shown that individuals frequently accept the stated principles of political parties they strongly disapprove of so long as they are unaware that these principles have the endorsement of those parties (60). Cantril and Harding discovered that over half of the voting population of this country were completely unable to tell what the differences were between the major political parties even though the overwhelming majority of these same people voted consistently for one or the other of the major political parties (31). Their analysis of the Congressional election of 1942 shows that the overwhelming proportion of the major issues of the day which might logically be thought to have some relationship to the way a person voted and which were discussed during the election campaign as partisan issues actually had little or no importance in determining vote. They also showed how people rationalize their accepted attitude by claiming that they usually vote for the candidate rather than for the party that candidate represents. At least two-thirds of the people who vote a straight party ticket claim that they vote for the man rather than the party. Even the minority of voters who classify themselves as 'independents' appear, on closer scrutiny, to be considerably less independent than they claim. At least half of the 'independents' vote a straight party ticket and well over half of the 'independents' vote according to their father's political affiliation.

Lazarsfeld, Berelson, and Gaudet have shown that a fairly reliable index of 'political predisposition' can be constructed for an individual from a knowledge of three social factors: religious affiliation, economic status, and place of residence (88). They found that cross pressures of these three social factors tended to delay a person's decision as to how he would vote. Relatively few voters were sufficiently affected by political propaganda to change their political predisposition. Persons tend to expose themselves mainly to the politi-

cal propaganda of their own party but those whose predisposition is toward one of the major parties and who expose themselves more than others of their persuasion to opposition propaganda, do tend to vote more than others for the opposition party. The less structured a person's political attitude is—either because of lack of interest or social pressures—the greater is the variability of voting behavior, the greater the influence of propaganda from various media or by personal contact.

Breslaw concluded from his detailed interviews concerned with the development of political attitudes that political attitudes can more appropriately be described as an 'orientation' or 'bias' than a 'point of view' logically arrived at (17). "Attitudes emerge from the particular social life which happens to surround the individual" (p. 65). An attitude "is an end product with no necessary relationship to the particular components of that stimulation. An attitude is something that becomes implanted—as fear of the dark becomes implanted—in many different ways" (p. 66).

The consistency with which an individual's attitude determines his reactions to situations to which he relates the attitude has been shown in a number of different studies. Vetter found that a person who was radical or conservative tended to be radical or conservative in his reaction to a wide variety of social, political, and ethical situations (127). Katz and F. H. Allport noted the "consistency with which different attitudes seem to fit together in their respective patterns" (72, p. 48). Cantril demonstrated that an attitude has a directive influence on numerous specific reactions to which it is related, that an attitude tends to be enduring and constant even though the situations which evoke it may fluctuate, and that an attitude can be quite independent of the particular experiences which may have established it (19). Stagner demonstrated that persons who are intensely chauvinistic tend to react consistently and unfavorably to a wide variety of issues such as tariff reduction, labor unions, or government ownership (119). Analyses of data obtained from public opinion surveys show that in the summer of 1940 various attitudes of the American people toward the war in Europe tended to cluster into three broad, underlying patterns (29).

In a study of the reaction of the American people to World War II, we find that while the religious frame of Catholics previous to Pearl Harbor affected their attitude toward U. S. intervention in the European war, it did not affect their attitude toward U. S. intervention in the Pacific war—in the latter case there was no conflict between religious and nationalistic attitudes influencing the judgments of a stimulus situation (34). The conflict of attitudes in German-American and Italian-American citizens resulting from U. S. intervention in World War II and the effect of this conflict in voting behavior is revealed by the findings of Bean, Mosteller and Williams that these two groups significantly shifted their vote away from Roosevelt in the 1940 election (8).

Analysis of data obtained from public opinion polls shows that major events are judged in terms of frames of reference which enable people to relate these events to their own self-interest. Hence public opinion on specific issues is highly sensitive to events (33). Polls further show that once the vast majority of individuals become aware that a social, economic, or political problem exists, they develop attitudes toward that problem—the proportion of people who remain neutral or who have no opinion about an issue they are aware of is very small indeed.

Studies on the prediction of social events have shown the enormous extent to which attitudes direct the way in which the future is projected (25, 94). A person's attitude toward socialism, for example, largely determines his picture of future economic trends. But the more highly structured and clear-cut the forces determining the resolution of an issue appear to be, the less is an individual's prediction determined by his own attitude. Predictions are especially influenced by frames or points of reference when the issue judged is ambiguous or unstructured because of the variety and apparent inconsistency of direction of the variables concerned.

Although the bulk of our attitudes do seem to be derived from the norms that surround us, the fact remains that—*all other things being equal*—individuals do acquire attitudes based on knowledge and reasoned analysis. Murphy and Likert found, for example, that, next to parental influence, a student's reading habits and scholarship tended more than other factors to affect his attitudes (98). Cantril has shown the tremendous difference between enlightened and unenlightened Americans in their pre-Pearl Harbor attitudes toward U. S. intervention in the war—with enlightenment being based on general knowledge and a feeling of a clear idea of what the war is all about (34). Differences of opinion according to the degree of 'enlightenment' were found within all income groups and were considerably greater than differences of opinion between income groups themselves. Evidence from public opinion polls shows that well-informed people accept less readily than uninformed persons many of the common stereotypes of the day, the facts they know serving as reference points for discrimination (139). Informed people are better able to see the implications of events and proposals to their own self-interest—they show considerably more concern, for example, in international affairs as contrasted to relatively uninformed persons whose predominant concern is with strictly domestic problems (35). The uninformed group generally has a comparatively higher 'no opinion' reaction to most public questions. Differences of opinion according to the amount of information a person has do *not* appear, however, with respect to issues where wish-fulfillment is clearly involved. For example, a study of the relative importance of education and economic status in determining opinion showed that education was more important than income only in situations where greater knowledge gives an insight into the effect of certain events or proposals that do not deal with an individual's financial return in any clear-cut way (140). Diven's analysis of aesthetic appreciation indicates that—all other things being equal—if a well-informed and an uninformed person hold the same attitude with equal intensity the essential difference between them is that the informed man will be better able to rationalize his point of view, supporting it with what seems to him sound evidence (45).

Cantril's study of the nation-wide panic resulting from Orson Welles' broadcast 'War of the Worlds' showed that when individuals are faced with a critical and apparently dangerous situation they readily accept any interpretation offered them as a basis for judgment if they have no appropriate and sure standards by means of which to evaluate the situation (28). Those frightened by the broadcast were highly suggestible, believing what they heard to be true and being unable themselves to make any external or internal checks of the reports. Furthermore, for most of those who became panicked, the story was credible since it fit into preëxisting attitudes such as the belief that God

would someday destroy our planet, an attack by a foreign power, or fanciful notions concerning the possibilities of science. People who lacked appropriate standards to interpret the broadcast properly were found particularly among those who had neither the opportunity nor the ability to acquire information or training that would have protected them with relevant points of reference. This broadcast occurred in the fall of 1938 when vast numbers of the population were unusually bewildered by the prolonged economic insecurity they had experienced and by the precarious and delicate state of world affairs following the Munich settlement. In commenting on the panic, Heywood Broun tersely and aptly summarized this effect of general, social and political unrest by saying that "jitters have come to roost." Current norms were somehow proving inadequate to account for objective conditions, the whole course of recent history created a relatively ambiguous and unstructured situation conducive to high suggestibility.

And just as critical conditions provide fertile soil for panics, so too do they provide the optimum conditions for rumor or for individual re-orientation by means of slogans or simple appeals. Knapp concludes, for example, from his study of rumors that "in proportion as the cognitive world is ambiguous or ill defined and the motivation intense, rumors will find life" (79, p. 31). F. H. Allport and Lepkin in their study of rumors point out that "the more 'outer' facts, or true reports of facts, the individual has within his grasp, and the more he is stimulated to weigh this evidence objectively, the more nearly the picture he forms 'in his mind' will conform to the true reality, and the less altered it will be through the effect of emotion and impulse" (1, p. 14). And Sherif points out from his analysis of slogans "that slogans are short-cut expressions arising in confused and critical situations . . . the more correctly and the more objectively a set of slogans expresses the underlying forces in a critical situation, the more vital and lasting they will prove to be" (116, p. 461). In a more recent study of slogans, Bellak reaches the same conclusion with special emphasis on the rôle of motivational factors (10). Since slogans can only be expected to take hold in critical times when they give meaning to or point a way out of confused situations, it is not surprising that the judgments of slogans in the laboratory are unaffected by conflicting standards as in the experiment of Asch, Block and Hertzman (3) or Block's later study where only restricted shifts were found in the ratings of slogans when authoritative standards were imposed under laboratory conditions (89). The leaders of any mass movement or revolution show sound psychology when, during a critical situation where old norms have lost their hold, they try first of all to get control of the mass media of communication so they can issue new instructions, spread slogans, and otherwise try to restructure people's thinking.

Cantril's analysis of the rise of various social movements shows how persons dissatisfied with their status or the fulfillment of their needs tend to accept new frames of reference provided by a leader or nuclear group which seem to them to 'explain' their situation more appropriately and to offer an apparently more effective course of action than did adherence to the more commonly accepted norms of the social system (30). The suggestibility of a person to new norms was found to be proportional to the inadequacy of existing standards for the interpretation of particular situations, to his desire for a more adequate interpretation or to the ease with which

the norms of a new movement could be related to established frames.

All of the representative studies cited above indicate that only by means of some frame or anchorage can and does the individual judge and react to social stimuli. Individuals in social life cannot long remain normally adjusted if they are in a state of indecision. Sooner or later they must and do make some appropriate judgment or reaction to a stimulus, place it in some way meaningful to them. The psychological process they use to make such judgments and give meaning to their social environment is to refer the stimuli around them to some frame of reference or anchorage they acquire and which is a readiness for reaction. These frames or anchorages which make everyday social judgments possible are, like frames or anchorages discovered in laboratory experiments, inevitably developed with repeated exposure to the same stimuli (objects, persons, groups, values, or norms) and individuals are by no means always aware that frames or anchorages have become established.

SUMMARY

The established conformities in the experience and behavior of individual members of a group may be expressed in an empirical way and on the psychological level as the effects of established attitudes. Because of this, 'attitudes' become a major meeting ground for psychologists and sociologists. In spite of the vast literature on attitudes contributed by both psychologists and sociologists, there is as yet no recognized psychology of attitudes with basic concepts applicable to all cases of attitudes. In large part, this may be due to the fact that the term 'attitude,' though useful as an empirical and composite term, is not yet delineated by underlying psychological concepts. An attitude does not designate any specific psychological principle in operation.

Psychologically, an attitude implies an *established* state of readiness. But the cases of psychological readiness are not exhausted by all cases of attitudes. An attitude has certain characteristic features or criteria which differentiate it from other states of readiness: an attitude is an established readiness which has a subject-object relationship of highly variable content, which is learned (formed), has affective properties with various degrees of motivational components, may refer to whatever stimuli are encompassed in the subject-object relationship, and which determines that an individual will react to a stimulus in a selective way.

Thus characterized, the psychology of value is included in the psychology of attitudes. And since there are states of readiness possessing the above criteria and established in strictly personal situations, all attitudes are not shared by other members of the group (individual attitudes). Hence the meaning of a statement above that the established conformities of members of a group may be thought of as the effects of established attitudes should be modified accordingly. Since there are as many different kinds of attitudes as there are stimulus situations or norms to which they are related, a rigid classification of attitudes becomes meaningless.

The first stage in the actual formation of an attitude is a perceptual stage and the psychology of attitudes is related to the psychology of perception. And since attitudes are inferred from the *selective* nature of response (verbal or non-verbal), the psychology of judgment also lies at the basis of attitude formation. Perceptions and judgments take place in referential frameworks. The properties of perceptions and judgments are determined or altered by the

properties of the scales or frames to which they are related. An individual inevitably forms such scales or frames if he faces a stimulus situation repeatedly. He may or may not be consciously aware of this fact. But once such scales or frames are formed, he reacts to subsequent stimulation by related stimuli in a characteristic or selective way. In other words, these established scales or frames serve as anchorages to structure or modify subsequent experience or response.

In cases where the stimulus field is well-structured, the frames or scales formed are determined in a compelling way by the objective properties of the stimulus situation and correspond to them. The frames formed by the products of technology are of this kind, determining the mentality of individuals to an important degree. In cases where the stimulus situation is not well-structured, the psychological result is not chaos but again the formation of a frame. In these cases, internal factors (*e.g.*, temperament, motivation) and social factors (*e.g.*, suggestion, prestige and other social influences) contribute in an important degree to the structuration of the frame. The rôle played by internal and social factors decreases with the stability, unambiguity, or structuredness of the stimulus situation, and increases with its instability or ambiguity. But whether the stimulus field is structured or not, the result of repeated stimulation is the formation of a frame.

Studies on perception and judgment furnish ample experimental evidence of the formation of scales and frames of reference and the same basic psychological principles are operative in cases of frame formation where social factors play an important rôle. The effects of anchorages which come in to structure or modify experience are demonstrated with social factors of varying degrees of concreteness as well as with non-social factors. Individuals in social life are sooner or later bound to make some decisions with respect to a stimulus situation that confronts them. They do this by referring the stimulus to some frame or standard. Whether or not the individual is aware of it, these frames or standards invariably develop in social life with repeated exposure to the same stimuli. Many of these frames have all the characteristics of attitudes and, once formed, attitudes act as anchorages to related situations and thus have a selective property as inferred from verbal or non-verbal responses.

The more ambiguous and unstructured the social stimulus situation is, the more will established frames or anchorages exert an influence in determining reaction. If the stimulus situation is ambiguous and there are no established frames to which it can be related, new norms will be readily accepted and will effectively structure people's thinking. In concrete social life, critical periods of unrest, such as prolonged depressions or revolutions, are examples *par excellence* of ambiguous, unstable situations. During these times, old norms and values are inadequate and new anchorages, such as slogans, provide an apparent basis for judgment or action. This new formulation may or may not account for the critical situation objectively. But the study of the adequacy and validity of the formulation is one that goes beyond psychology alone.

The content and affective properties of attitudinal anchorages will vary according to the motivational and other factors involved in the specific case of the subject-object relationship. In most concrete social situations of everyday life the contents of attitudes have affective, motivational, or status-involving factors of varying degrees of intensity. Whether the perceptual stage in the

formation of a social attitude is based on direct contact with a person or group, or whether it is based on the transmission of a social norm by short-cut verbal value-judgment makes no difference psychologically. Hence the basic psychological properties of attitudes—quite irrespective of what the attitude is concerned with or whether it is studied by the experimental psychologist or the sociologist—are essentially the same.

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PSYCHOLOGICAL TESTING IN MILITARY CLINICAL PSYCHOLOGY: I. INTELLIGENCE TESTING[1]

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The question is sometimes raised as to whether psychological testing has made the same advances in this war that it made in the last. If one remembers the immature state of group testing at the outbreak of World War I and its tremendous growth during that conflict, the answer must be 'no.' World War II has offered nothing as novel or as spectacular as that. This is no reflection on the contribution of psychometrics in the present conflict, but represents the fact that this time there was an established, highly-developed body of testing material ready for use. The present contributions have been in the relatively unspectacular direction of adaptation and development rather than invention. Clinical testing has followed the same pattern. Largely undeveloped at the time of World War I, it grew to full stature during the subsequent years of peace. When hostilities broke out this time there was available a rich body of clinical testing techniques ready to be adapted to military needs.

Certain emergency conditions attributable to our lack of preparation for war also encouraged the expenditure of our professional energies in the direction of adaptation rather than invention. American psychologists in general did not foresee the inevitability of our being plunged into war, and had taken few anticipatory steps toward meeting the peculiar problems of military testing (19). As a result those of us who found ourselves in uniform at the beginning of the emergency were faced with the problem of practicing our profession without there being available any special techniques fitted to the novel conditions presented to us. We were forced to work and carry a full load of testing at the same time that we were developing whatever new tools were needed. Under these conditions it was only natural that adaptation rather than invention should be the order of the day.

In our discussion of clinical testing within the military services we will devote the present article to developments in intelligence testing, leaving personality tests to be treated in the article to follow. The military services have contributed no truly novel approaches to the problem of intelligence testing. Where new tests have been developed, such as the Army Individual Test, they have relied upon adaptations of the classical types of test item. What has been contributed has been in the nature of a refinement of present techniques and a fuller understanding of previously existing problems. This fuller understanding is attributable in no small part to our opportunity to test large groups from all strata, geographical and cultural, of our population.

One of the most important contributions from the services has been the development of abbreviated testing techniques. There are many situations in military practice where an individual test of intelligence is desirable, and even

[1] The opinions or assertions contained herein are the private ones of the writers and are not to be construed as official or reflecting the views of the Navy Department or the Naval Service at large.

SUMMARY OF ABBREVIATED INTELLIGENCE TESTS USED IN THE NAVY

Test	Reference*	Subjects	Validation Criterion	Validity Coefficient
Kent E. G. Y.	Lewinski (14)	290 Naval recruits	Wechsler-Bellevue Verbal Scale	0.727
Revised Kent Emergency	Lewinski (13)	200 white Naval recruits	Original Kent E.G.Y.	0.811
CAS Scale (Wechsler-Bellevue Comprehension, Arithmetic, Similarities)	Springer (18)	100 Naval recruits	WB Verbal Scale WB Performance Scale Full WB Full WB—CAS	0.96 0.80 0.92 0.89
Differentiation Test	Colemen (2)	100 inductees, 5,787 Naval recruits	Wechsler-Bellevue Verbal Scale Distribution of scores published and can be used as normative standards	
Herring Binet (Group A, mean time of administration 6.07 minutes)	Lewinski (12)	100 Naval recruits	Wechsler-Bellevue Verbal-Scale	0.654
Kent Battery of One-Minute Tests	Kent (10, 11)	1,915 public school pupils	Distribution of scores published and can be used as normative standards	

* Number following author's name indicates reference in bibliography.

necessary, but where the exigencies of time and the large volume of men to be handled render it impossible to use the time-consuming longer test forms of standard clinical usage. This is not only true in screening large numbers of men in psychiatric selection at the recruit level, but also in the clinical evaluation of the psychiatric casualties passing through our military hospitals. As a result numerous abbreviated testing techniques have been developed and have proven satisfactory. The table lists some of those in use in Naval installations. Although most of them have been validated on recruits during selection, there is no reason to assume that they will not work equally well in hospital practice. A recent unpublished study at the U. S. Naval Hospital at St. Albans, using Rabin's (15) CAS abbreviation (comprehension, arithmetic, similarities) of the Wechsler-Bellevue, has given results on 100 unselected neuropsychiatric cases[2] that compare favorably with those of Springer included in the table. Such techniques have more flexibility and greater potentiality than usually is realized.

The Psychiatric Unit at the Newport Naval Training Station, using elements from the numerous short tests developed by Grace Kent, was able to put together a brief battery which could be administered in from eight to ten minutes (9). This battery not only covered a fairly wide range of intellectual func-

[2] Personal communication from Lt. Frances B. Holmes, H(W), USNR.

tions, but included both verbal and written sub-tests, thus acting as a screen for illiteracy as well as mental deficiency. Using this battery, experienced clinicians were able to predict the Wechsler-Bellevue mental age with an average error of six months or less.

Such short tests will undoubtedly be useful in civilian clinical practice. War has made the public acutely conscious of psychiatric problems and it is only wise to anticipate an increased demand on mental hygiene facilities following the cessation of hostilities. These demands will find psychology suffering from a shortage of experienced personnel, a shortage aggravated by the manpower policy of cutting off the supply of all able-bodied young men to our graduate schools. The situation in our medical schools is also acute. The military authorities, however, have demanded that at least a minimum of young men be trained in medicine in order to supply the demands of the armed forces. No such policy has been established for psychology, and were the war to continue for any great length of time there is serious question as to where the necessary replacements would be obtained to continue the clinical psychological services within the armed forces. Under these circumstances, if any expansion of clinical facilities in psychology is to take place, either in civilian or military circles, much of the extra case load involved must be handled by streamlining and abbreviating our present testing techniques.

There are three common arguments against the use of shorter forms. First, they do not offer the fineness of discriminatory measure that the long tests do. Second, they do not offer the same richness of diagnostic possibilities, *i.e.*, in the analysis of scatter. Third, their use demands more clinical background and skill on the part of the examiner. There is truth in all these arguments but they are not as conclusive as they seem.

Fineness of discrimination is not as possible with the shorter test forms as it is with the longer. The ten items and thirty-six possible points of credit of the Revised Kent Emergency Test or the thirty-two items and fifty-four points of credit of the CAS abbreviation of the Wechsler-Bellevue cannot be expected to offer the differential discrimination of the 129 items of the revised Stanford Binet. We will not discuss the critical literature dealing with practices where numerical measures are carried beyond the discriminative power of the data on which they are based. Instead we will confine ourselves to pointing out that there are many situations where fine discrimination is not necessary. Seldom does the clinical psychologist dealing with adults find differences of less than six months in mental age or of five points in I.Q. of decisive importance in interpreting a case.

Screen testing as a military selection procedure is one situation where rough measures are equal to the needs of the situation. As the word 'screening' implies, selection is a sifting process and in practice usually begins with the use of coarse measures which are increasingly refined as the process develops. There are many stages of selection for intelligence where a single item will serve to differentiate those men who may be passed on as intelligent enough for service from those men who need further study. This last group may then progressively be submitted to finer measures as they are needed. We have found that a single question from the revised Kent Emergency Test may be enough to determine whether a man may safely be passed for duty. If he can answer correctly the question "If your shadow points to the northeast, where is the sun?", the chances of his

being mentally deficient are practically nil. If he fails this question, however, it is still necessary to give him further testing to determine mental deficiency, since many people who cannot answer this question are not feebleminded. At this point a brief battery can be introduced to determine whether or not the man should be held for further observation. Hildreth has discussed the philosophy and techniques of such progressive screening (4, 5). No recruit is separated from the Naval Service on the results of such brief measures alone, but only after the administration of a complete, standard test given him while he is on the observation ward. The longer test is often not necessary to establish mental deficiency but contributes further validity and assures the recruit the benefit of every doubt before discharge. There are many civilian situations both in clinics and institutions where such screening procedures could be instituted with profit.

In the clinical evaluation of many psychiatric cases, some understanding of the patient's intellectual capacity is desirable. Intelligence level may contribute to such things as the basic sources of insecurity, the development of environmental tensions, the nature of conversion symptoms, and the level at which therapy must be conducted. In most cases, no fine measure of intelligence is necessary, and a short form of the Wechsler-Bellevue, for instance, will contribute as much as the long form. Here the use of abbreviated techniques can do much to lighten the burden of routine hospital testing. None of these arguments should be interpreted as denying the value of discriminatory power in our measures of intelligence nor the necessity of using the longer test forms in certain cases; we merely wish to point out the many opportunities where brief tests can be substituted with a consequent saving of valuable time and labor.

The claim that abbreviated test techniques do not offer the diagnostic material available in the longer forms is based largely on ignorance rather than on negative findings. While we assume that the more material our test contains the more opportunity there is for 'scatter' (intra- and inter-test variability in performance) to show itself, no careful study of scatter has been made with brief tests. There is one preliminary study which indicates that scatter may show up on such brief forms (8). Were we to design our short tests with 'scatter' in mind, their diagnostic possibilities might well be increased. The Shipley-Hartford Retreat Scale (17) is one instance where a shortened technique has worked very well in the detection of deficit (impairment, deterioration). The truth is that until the exigencies of military testing forced us to consider economy of time and labor, we paid little attention to such matters in developing tests. Test efficiency in the past has been measured largely in terms of the goal attained rather than the effort expended in attaining it.

It is true that successful use of abbreviated techniques demands clinical skill and experience on the part of the examiner. Such factors as rapport between examiner and subject assume even greater importance when a brief test is used. The possibility of error due to chance factors is much greater, as the study of the relationship between reliability and number of test items has shown us. We have said that in the hands of a skilled clinician the results obtained with the brief Newport battery closely approached those obtained with the complete Wechsler-Bellevue, but we should also mention that in the hands of a novice the discrepancy could be great. If brief tests are to work, every

item on the test must be selected only after careful trial, and the final test evolved must be administered by a skilled examiner. In competent hands, with due regard to the factors discussed here, the shorter test forms can be an important part of the clinical psychologist's armamentarium.

Clinical experience is also necessary in the administration and interpretation of the longer test forms. This is often disregarded. The goal of the whole psychometric movement has been the construction of a completely objective, carefully standardized technique that operates almost on a mechanical level. If this goal were attained, clinical judgment would become unnecessary, the examiner an inconsequential cog in the mechanical administration of the test, and the test result valid and reliable without further interpretation. Two things are wrong with this picture. In the first place all the statistical procedures involved in test validation are group procedures. They are based on a group tendency and allow for a margin of error which expresses itself as a dispersion around the central tendency. Ideally, this margin is never large for many cases. The statistician therefore writes it off as statistically unimportant, and the psychologist interested in group testing writes it off as operating expense. The clinical psychologist, however, is not dealing with groups, but with individuals, and they are individuals who usually come from the deviant group, the atypical persons to whom a group-validated test does not do justice. In handling these deviants, any standardized testing procedure frequently will yield erroneous results which can only be corrected by the psychologist's clinical evaluation. In the second place the goal of perfection has not been even approximated in many testing fields. Whatever dangers are inherent in applying group techniques to an individual (and this practical statistical problem extends into the whole theoretical problem of the nomothetic versus the idiographic) are accentuated by the imperfection of most of our present techniques. The fact that psychometrics has perfection as its goal may satisfy our super-egos, but it should not blind us to the practical fact that this goal is far away at present.

As a result of our reliance upon objectivity and standardization, there is a growing tendency for the clinical psychologist to relegate his testing duties to a subordinate position. Since the objectivity and standardization of psychological tests supposedly eliminate the need for subjective evaluation, some psychologists feel that tests can be administered by a relatively untrained person, and there has been created a whole category of 'psychometricians' subordinate to, and less well-trained than, the 'clinical psychologist.' This may be a necessary expedient in a rapidly expanding profession where there is a real shortage of adequately trained personnel, but it seems dangerous to accept it as more than a temporary expedient, and to write such a provision into our permanent professional structure. In the course of our Naval experience we have had the opportunity of comparing our results on many patients with extensive case histories which included materials furnished by the various social agencies with which these patients previously had come in contact. It has been disturbing to see how often inadequate testing has resulted in a misunderstanding of the patient's basic difficulty. The most frequent error has been found in the handling of the dull psychopath in our school systems. Apparently the poor rapport typical of the psychopath has resulted in frequent under-testing with the consequence that many psychopaths have

been classified fallaciously as mentally deficient. Many organic and psychotic cases also have been placed in the feebleminded category, apparently because the examiner lacked the background to interpret the scatter pattern of test scores.

The necessity of supplementing intelligence test results with a clinical evaluation is reinforced by certain specific deficiencies which appear in these tests when they are applied to a military group. In validating new tests we try for a wide sampling of people, a group that is truly representative of the country at large, but this ideal is seldom obtained. We are forced to rely upon the available material which is largely taken from urban educational populations. When war arrives and the democratic procedures of selective service provide us with a truly unselected, random population representing the United States as a whole we find ourselves with testing instruments that are not fitted for many of the men to be tested owing to the presence of educational and cultural handicaps.

The test constructors are not entirely to blame for this situation. To validate a test one needs large numbers of subjects who are willing to coöperate. Such large numbers are usually obtained through the public school system. It is only in the larger educational systems that the student population is sufficiently concentrated to render coöperation efficient. This limits the sampling to those geographical areas that are relatively culturally enlightened and educationally advanced. Moreover, it is only in these same enlightened areas that the necessary coöperation is forthcoming from school authorities and institutions. Such selection biases the tests in favor of cultural strata that are not typical of the country as a whole. Once the tests are validated and made available for use, they in turn are made use of only in these same communities, since these are the only ones sufficiently advanced to provide the educational and clinical facilities using such tests. The result is a sort of cultural inbreeding in our measures of intelligence. Naturally these measures appear satisfactory for they are used only in those groups on which they were validated.

War changes the situation, and we suddenly find ourselves faced with the problem of testing people from every geographical area and every cultural level. In consequence our tests are not adequate to the demands placed upon them. They do not work well with rural subjects whose educational opportunities have been limited. They do not work well with culturally isolated and impoverished foreign language and racial groups. Yet these people loom large in our military population. Supposedly performance items, as opposed to verbal items, are included in our tests to cover just these problems of educational and cultural handicap, yet it has been our experience that the standard performance items such as those in the Wechsler-Bellevue Scale are as badly culture-bound as are the verbal items.

Nowhere is this so evident as in testing the Southern Negro. Our present tests are not devised for him, and consequently they will not work well with him. The outstanding example we have met was a Southern Negro twenty-nine years old who, despite the economic handicaps and social restrictions under which he lived, was able to acquire a ten-acre truck farm, a house, a Ford truck, and a touring car. This man supervised the growing of his vegetable crops and their disposition in a nearby city, yet was unable to obtain an intelligence quotient above fifty when tested on the Wechsler-Bellevue. Surely this man was not an imbecile! The unsuitability of the standard tests when applied to Negroes

has been demonstrated over and over again in our experience in the Navy, and the Army has also found that tests standardized upon a white group do not predict as well for a colored group. In view of these findings the recent revival of the argument over the relative intelligence of white and Negro groups appears slightly ludicrous, since the test results on which the argument is based are hardly appropriate to the question. One may say that Negroes do not do as well on tests designed for a white group as do the whites, but beyond that one cannot go with safety. In applying our standard intelligence tests to the Southern Negro, we are still measuring one type of actual achievement rather than true potentiality.

With test performance so intimately tied to educational experience and cultural background it is easy to see how the I.Q. can change in some cases, when a changed environment results in stimulation and educational enrichment. The intelligence quotient is a direct function of the tests from which it is derived and these tests are loaded with educational and cultural factors. In groups where educational and cultural standardization offer opportunity to all, these factors are equated for all members of the group and thus held more or less constant, enabling us to approach our goal of measuring potential capacity. Where educational or cultural deprivation enters in the individual's history, opportunity is no longer held constant for all members of the group, and the test then becomes more of a measure of experience than of potentiality. Under such circumstances a change in environment should affect the I.Q. In the case of the individual mentioned in the previous paragraph, few psychologists would hesitate to guarantee a substantial increase in test performance with a year or two of intensive formal education, which is the equivalent of saying that in the case of that individual the intelligence quotient could be raised. Constructing tests that take advantage of common cultural background is not the same thing as building tests that are genuinely culture-free. Such tests may act like culture-free tests when used within a group all of whom have had the same experiences, since commonality cancels out the effect of culture, but the fact that they actually are culture-bound rather than culture-free becomes glaringly evident when they are applied to deprived individuals who have not had the typical group opportunity.

Implicit in these last remarks is the assumption that the aim of intelligence testing has been to measure capacity or potentiality rather than achievement. The truth of this statement would seem to be borne out by the separate development of intelligence tests and achievement tests. Whether or not we do measure capacity or potentiality, and whether this capacity or potentiality is of an innate nature is still open to question. Chein (1) has recently discussed this same point. It may well be that the whole conception of innate capacity is nothing more than a convenient abstraction that we use in our thinking, and that the goal of testing such potentiality on a relatively pure level is impossible of attainment. Certainly we have not achieved it to date, although in some instances a fair approximation seems to have been reached. Such 'pure capacity' may exist only on a conceptual level, and in actually measuring intelligence we may only grasp it through some intermediary behavior; but in those cases mentioned above where education and culture are held constant, differential measures are then attained which would seem to be approaching our goal. In those cases where the intermediary functions through which we measure intelligence are not controlled, as they certainly are

not in the case of educationally and culturally handicapped individuals, we fall far short of our ideal.

These remarks have some bearing on the question of what criteria we should use in validating our tests of intelligence for service use. In military practice we validate against two types of criteria. The first is some standard or accepted measure of intelligence such as another intelligence test. Opposed to this we may accept as a criterion some measure of success within the military services, usually the ability to complete successfully basic training.

This second criterion, the ability to complete successfully basic training, is not a function of intelligence alone. Many other factors enter into the picture. Emotional stability, total personality structure, past training, cultural background, etc., are all vital factors in adaptability to the military service. While pragmatically we may be interested in suitability for service rather than intelligence per se, lumping all these basic factors together under the single criterion of successful adaptation to service results in a genuine confusion in which we are unable to isolate and individually evaluate the specific elements. If our selection test is validated against the ability to complete successfully basic training, our test then becomes only in part a test of intelligence. Since intelligence is a recognized factor and one which is written into the requirements for military selection, as stated in the Manual of the Medical Department of the U. S. Navy, it would seem wisest to keep it separate, and avoid the confusion which arises when we validate a test directly against military performance.

A more careful examination of the criterion of completion of basic training will reveal that in itself this criterion is of dubious validity. The assumption is that such ability represents a capacity or potentiality of the individual, but this assumption is only partly correct. The successful completion of basic training is a function of many other extraneous factors which do not depend upon the individual's ability. Such completion of training depends in the first place on the availability of suitable administrative channels for discharge. A man may successfully complete his military training and indeed his entire military career simply because there is no convenient way of getting rid of him. In the early days of this war before the adoption of some means of administrative discharge such as the present practice of releasing men for inaptitude, many morons and inefficient individuals were kept on in the service simply because there was no way of getting rid of them once they were in. It was this situation which resulted in the Navy's adoption of the inaptitude discharge in 1941. As long as such a situation exists, the criterion we are discussing is certainly impure in any scientific sense. In addition the evaluation of success within the military service is too often left to the personal judgments of superior officers—judgments which are based on fluctuating factors and which are not universally anchored on adequate objective measures. Finally, whether or not a man is allowed to complete his basic training is a direct function of the quota system arising from manpower needs. Adequate performance today may be an utterly different thing from adequate performance tomorrow or yesterday, depending upon the current status of the manpower shortage. With these factors in mind one cannot help wondering about the serviceability of such a criterion of validation.

In favor of keeping intelligence as an independent factor to be evaluated in the selection of men, we would mention the following points. Having a direct

measure of intelligence is closer to the spirit of the various medical manuals setting forth the requirements for acceptance in the armed services, since these standards make open reference to intelligence per se. With some separate evaluation of intelligence available it is possible to control this factor and therefore gain a more definite knowledge of the influence and importance of the other factors entering into military adaptability. Finally, it makes possible a comparison of our findings with those which have accumulated during our study of intelligence in other fields.

No discussion of military testing can overlook the use of intelligence tests, not as measures of intelligence per se, but as diagnostic indicators of psychiatric conditions. At the training station level, where military selection is all-important, measuring intelligence for its own sake is of great importance, but in the military hospital the intelligence test is much more apt to be used for its clinical diagnostic possibilities. The analysis of scatter was firmly established in civilian practice before the war broke out but its importance to military clinical practice has been a great impetus to its further study. Thus the results of Rapaport's recent investigation of the subject have been widely circulated by the Macy Foundation in order to aid military psychologists in their understanding of this field (16). While the general value of Rapaport's study must be recognized, it is unfortunate from the standpoint of the military services that it was done on selected groups which are not typical of the wide range of military personnel. The group of patients used probably is, and the control group used definitely is, selected from a level of intelligence and cultural background that is too high to be representative of the military services. It is also unfortunate that no group of mental deficients was included in the experimental group, since the differentiation of mental deficiency from other clinical conditions is an important part of the work of the military psychologist.

In our study of the diagnostic possibilities of scatter we have as yet only scratched the surface and practically nothing is known about the possibilities of scatter on abbreviated test batteries of the sort we have mentioned above, although one study has indicated its possible use with such tests (8). Moreover, our study of scatter has been too closely linked to the individual intelligence test, particularly the Wechsler-Bellevue because of its primary suitability for such investigation. We should know more about the use of scatter on paper and pencil group tests and about the possibilities of comparing different test scores within larger batteries, *i.e.*, the comparison of group test performance with individual test performance. Such studies should aid us in further understanding the causal agents responsible for scatter. Thus in studying scatter on an abbreviated test battery, Hunt and Older (8) found significant differences between the psychopath and other clinical groups in their performance on verbal versus written tests. The factor involved here seemed to be that the psychopaths performed better when allowed to work by themselves, as they do on a written test, than when they have to meet the continual social interchange between the examiner and subject which is a requisite of the administration of individual verbal tests. This suggests that the scatter previously found on individual tests where the performance test scores of the psychopath often run higher than his verbal test scores, is not due to a difference in verbal ability versus performance ability but rather to the fact that most performance tests (and we might mention the Wechsler-Bellevue

digit symbol, Kohs blocks, and picture arrangement as examples) allow the individual the same relative chance to 'work alone' that is offered by a written test. It is quite possible that scatter in this situation may be related to the fact that the psychopath's known difficulty in getting along with others influences the factor of rapport between examiner and subject.

Future investigations of scatter could be improved by more attention to experimental controls. The factor of test administration has not received adequate attention in the past. Sometimes the tests used in scatter studies have been administered by one examiner, sometimes by a few, rarely by many. The assumption has been that the factor of test administration is sufficiently controlled by the use of standard instructions in administration and by an assumed common level of professional training on the part of the examiners. Actually these vary considerably and their influence on scatter is unknown. Moreover the personality of the examiner may influence the test results. By holding constant the examiners' professional training and their methods of test administration we have been able to demonstrate one type of scatter in which performance scores are definitely higher than verbal scores (commonly thought to be a type of scatter typical of the constitutional psychopath) as a function of the examiner giving the test. That this probably was attributable to the inability of this particular examiner to obtain rapport in the testing situation is not pertinent to our discussion. The important thing is that such influences can be demonstrated. We should exercise more care in controlling the element of test administration when studying scatter. This could be done in one of two ways, either by empirically establishing the influence of each examiner and then taking account of this factor in evaluating the results, or by using a sufficient number of examiners to insure a random sampling. So far this particular control seems to have been overlooked.

In the second place there has been too much uncritical acceptance of diagnostic criteria. Everyone would agree that schizophrenia as a single term covers a multitude of widely varying types of clinical behavior. And yet the simple diagnostic category 'schizophrenia' or 'constitutional psychopathic state' is often used as a criterion for classifying groups for study. Rapaport's study shows an encouraging attempt to deal with this problem. Some clinical description of the cases used is needed to make possible a more discriminative classification. Since diagnostic practice also varies with the individual psychiatrist giving the diagnosis, it might be wise to introduce some control of this factor by insuring a random sampling of psychiatrists in the selection of those groups studied. Finally, test scatter is a function not only of mental disorder but also of educational and cultural handicaps, and these factors in turn demand further study in order that they may be isolated and controlled.

A further line of investigation that has developed from the interest in scatter has been the study of malingering on intelligence tests. It was established by Hunt and Older (7) that the responses of the malingerer differ from those of the genuine mental defective and may be recognized easily. The peculiarity of response typical of the malingerer suggests the scatter exhibited by psychotics but has been shown to be distinctive (6). Using these differences Goldstein has constructed a malingering key for use with one of the Army selection tests (3). This practical application of the principles inherent in scatter has removed any serious threat of successful malingering on intelligence tests.

While military clinical testing in World War II may not have produced much that is new, it should certainly furnish some healthy stimulation for civilian practice. We feel that one of its greatest contributions has been the development of abbreviated test techniques. These economic measures not only have wide possibilities for use outside the military services, but their success should challenge us to reconsider the philosophy behind our construction of longer, more time-consuming tests. The opportunity that we have had to study unparalleled numbers of subjects, constituting a truly representative sampling of the population, has added to our knowledge of the potentialities and limits of our present techniques, and in many instances has provided a healthy sense of insecurity upon which to base a sceptical reëxamination of the present theoretical basis of intelligence testing. As usual when a social emergency drives psychology beyond its normal academic interests, some kind of growth is inevitable. In this instance we cannot help but feel that it will be a healthy growth.

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A STIMULUS–RESPONSE ANALYSIS OF REPRESSION AND INSIGHT IN PSYCHOTHERAPY

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A psychologist of the writer's acquaintance once remarked that he wished he knew what he meant when he said, "Counseling is successful when the client 'sees.'" He explained that the reason for his remark was that when the client 'sees' he is cured. Rogers has also paid his respects to the transformation which comes with the acquisition of insights gained in psychotherapy in the following way:

> "Whether we describe this change in terms of changed goals, changed motivation, release of repressions, or change in self-percepts, it is obvious that the change is a highly significant fact. It is a process of sufficient dynamic potency to command our full attention" (12, p. 194).

Probably few psychotherapists would be inclined to dispute the impressiveness of the changes accompanying the achievement of insight. In view of this, a recent paper by Mowrer and Ullman (11) takes on special significance since it offers clear suggestions as to what the psychologist quoted above 'meant' when he said, "The client 'sees.'" Although not primarily concerned with the problem of what happens when the client achieves insight, Mowrer and Ullman contribute an implication pertinent to this problem in the following statement (11, p. 81):

> "The common denominator in all . . . forms of non-integrative behavior seems to be the *inability to use symbols* appropriately as a means of bringing remote as well as immediate consequences into the present in such a manner that they may exert an influence proportional to their objective importance" (Italics the author's).

The implication, of course, is that non-integrative (neurotic) behavior may be eliminated through symbols becoming available. This implication is taken as a point of departure in this paper for purposes of analyzing the phenomenon of insight in psychotherapy.

Before taking up the question of how symbols become available and why this leads to the transformations accompanying insight, it will be helpful to consider the basis of their non-availability. One such basis is repression. Freud (4, p. 124) speaks of "the automatic mechanism of the pleasure-pain principle" being brought into play "and carrying through the repression of the dangerous impulse." The impulse presumably still exists, however. The clinical evidence for this, of course, has consisted of the disappearance of symptoms when the client gains insight into their underlying motivation. While such evidence is impressive, it does not afford an explanation of why the impulse should 'go underground' rather than dissipate. A recent study by Estes illuminates this problem. He has shown that while punishment may depress a response, "The extinction of a response which has been positively reinforced cannot be accelerated by the application of punishment; . . ." (2, p. 34). In terms of Estes' experiment 'repression of the dangerous impulse' means suppression of the act to which the impulse would have led, suppression resulting from punishment rather than removal resulting from extinction. The fact that the act is only suppressed indicates that the impulse to the act still exists. This analysis of repression

of impulses might meet with the criticism that human beings repress impulses when the behavior to which these impulses would lead has never actually been punished. A girl, *e.g.*, may repress impulses associated with a sexually tinged attraction toward her father when no actual sexual overtures toward the father have even been made. Human beings do, of course, learn to disapprove of certain impulses, however, and disapproval constitutes a form of punishment. Furthermore, human organisms learn to disapprove of their own impulses, and, therefore, punishment in the form of disapproval can operate to repress an impulse even when overt manifestations of the impulse have not been punished. Fisher furnishes the following striking illustration of the human organism's capacity to disapprove of itself:

"Some persons are so strongly identified with ideas of personal integrity and strength of character that they are intolerant of any fact which tends to defile their unblemished picture of themselves. They therefore do not wait to examine and then simply reject a motive or impulse which is antagonistic to their ego-identifications; they denounce and deny such a motive or impulse outright, as in the case of the young man who was discussing with his psychotherapist a dream which he had. Without preliminaries his psychotherapist asked him what his mother made him think of. Before the young man could catch himself he replied, 'A witch.' Whereupon he immediately leaped from his chair and violently disclaimed any such thought or feeling toward his mother" (3, p. 138).

The automatic mechanism of the pleasure-pain principle not only suppresses overt expression of the act, however, but also prevents the symbolization of the impulse to the act in consciousness. The reason for this, we believe, is that the disapproved impulse is a conditioned stimulus to the arousal of anxiety. Dissapproval, of course, arouses anxiety, and since the human organism is capable of disapproving its own impulses, the latter may also acquire the capacity for arousing anxiety. The organism is motivated to reduce anxiety and this can be achieved by removing the stimulus to the arousal of anxiety.[1] The non-symbolization in consciousness of an impulse which arouses anxiety constitutes removal of the impulse in the sense that it is not perceived. This can be expressed also by saying that the anxiety-evoking capacity of the impulse is reduced when the impulse is not perceived. To summarize, 'repression of the dangerous impulse' denotes not only suppression of overt expression of the impulse but also failure to perceive it (non-symbolization). Hereafter, 'repression' will be used in the latter sense.

The account of non-symbolization just presented has been used by the writer (14) to explain the results of a study in which it was found that favorable evaluations of the subjects were retained better than unfavorable evaluations. The interpretation given to these results was that disapproval, of which unfavorable evaluations are a form, is a powerful stimulus to the arousal of anxiety, and that any activity, including perceptual activity, which succeeds in getting away from the stimulus arousing this anxiety will be reinforced. Accordingly, unfavorable evaluations would not be retained as well as favorable evaluations which constitute secondary reinforcement.

Essentially the same conception of non-symbolization presented here seems

[1] This idea also originates with Mowrer: "This hypothesis . . . is that a conditioned avoidance response . . . is independently perpetuated, under favorable conditions, by the reinforcement resulting from the anxiety-reduction which accompanies termination of the conditioned stimulus" (10, p. 31).

to be implicit in the following quotation from Mowrer and Ullman:

"Freud has repeatedly remarked that repression of an impulse or memory characteristically occurs when it arouses effects which are so strong that they threaten to overwhelm the 'ego.' To this extent repression is definitely a 'defensive' mechanism. . . . Repression is effected by excluding the symbolic representative of certain impulses from consciousness, . . ." (11, p. 81).

Mowrer and Ullman go on to say:

"Although repression thus brings a momentary peace, the process is likely to be pathognomic for the reason that energies which formerly submitted themselves to the management of, and thereby strengthened, the 'ego' are now withdrawn and left free to seek—through those habits called symptoms—their own irresponsible, non-integrative paths to gratification" (11, p. 81).

The inference has already been drawn from another quotation from Mowrer and Ullman that 'non-integrative paths to gratification' may be eliminated through symbols becoming available. The reason for such elimination is also afforded by these writers:

"The fact that a given action often has not only immediate but also remote consequences and that, without the skillful use of symbols, the former tend to outweigh the latter is the basic condition of persistently non-integrative behavior . . ." (11, pp. 83–84).

The experimental work of Mowrer and Ullman becomes relevant to the present discussion at this point. After preliminary training during which animals learned to come to a food trough when a bell was sounded, an experimental period was begun. In the experimental period the animals were punished by shock if they ate before three seconds had elapsed. The animals were divided into three groups, the first receiving punishment immediately upon the termination of the three seconds, and the second and third receiving punishment three seconds and nine seconds, respectively, after the termination of the three seconds. The following comments are of particular interest (11, p. 77):

"In the course of exploratory work, we found that if, during the second stage of the experimental procedure, we continued to turn the buzzer off at the moment the food was presented (as we did during the first stage of training), our rats could not learn to behave integratively, regardless of whether the punishment came relatively soon or late. . . . In the main experiment as it was eventually carried out we therefore allowed the buzzer to continue to sound throughout the tabu period, *thus providing our subjects with a cue which was sufficiently conspicuous to enable them to make the necessary discrimination, . . .*" (Italics the author's).

The cue becoming conspicuous in this experiment affords a parallel to what happens in psychotherapy. Insight is gained into the 'dangerous impulse.' That is, it is symbolized in consciousness. The client now has a cue which was non-available previously because repression had made it inaccessible. The cue or symbol becoming available makes possible integrative behavior or 'cures' the client.

The contribution of Mowrer and Ullman to the psychotherapeutic phenomena under consideration here consists not only of their emphasis on the significance of cues or symbols in integrative learning, but also of their analysis of the time factor in such learning. There were three possible reactions which their animals could make: (1) taking the food within the tabu period and getting shocked, (2) avoiding shock by not eating at all, and (3) waiting throughout the tabu period and then eating thereby avoiding the shock but getting the food. The animals re-

ceiving punishment immediately upon the termination of the tabu period and those receiving it three seconds after the tabu period favored the last reaction, the most integrative of the three possible reactions, to a higher degree than those receiving punishment nine seconds after the tabu period. Mowrer and Ullman attribute this to the time factor, *i.e.*, an immediate reward (food) tends to outweigh a later punishment (shock) and the more remote the punishment, the greater the imbalance between immediate reward and later punishment.

Mowrer and Ullman summarize the importance of the time factor and cues in this way (11, p. 77):

"It will be recalled that we not only allowed the buzzer to sound throughout the tabu period but also, in the event of violation of the tabu, until the electric shock had come and gone. This latter provision was introduced as a means of helping our subjects 'make the connection,' or 'bridge the gap,' between what happened in the tabu period and what happened later by way of punishment. The impressive thing is that, even with these aids, the time factor was still so crucial in determining whether our subjects hit upon the integrative solution to the experimental problem or adopted a non-integrative adjustment. Unaided, the rat seems to be almost completely at the mercy of the gradient of reward and the gradient of punishment. *If a given response has two (or more) consequences which are separated by any appreciable time, this organism is evidently quite helpless, without external aids of the kind we supplied, to cope with the situation in anything but a stereotyped, non-integrative manner.* And even with such aids, the time factor is still of critical importance in determining the outcome" (Italics the author's).

Clinical examples of behavior resulting in immediate reward but leading to later punishment are given in the following paragraph:

"The child who resorts to enuresis as a means of venting resentment toward indifferent or harsh parents and thereby elicits still more rejection and loss of love; the business man who, when faced by the prospect of failure, becomes alcoholic and thus incapacitates himself for taking what might have been recuperative measures; or the woman who jealously nags her husband and in so doing destroys such residual affection as he may have for her—these are everyday examples of the self-defeating, shortsighted, non-integrative strategy which constitutes the 'vicious circle' and which seems to be a common feature of all psychoneuroses" (11, p. 83).

Our interest here, however, is in psychotherapy. Why is insight accompanied by transformations in behavior which replace the 'vicious circle'? We have already indicated that we believe the answer is that a previously repressed cue becomes available. The principle involved might be stated in this way: If a given action has immediately rewarding consequences as well as more remote punishing consequences, such action, other things being equal, will be abandoned only when a stimulus to the action also becomes a cue to later punishment or non-rewarding consequences. When neurotic behavior is eliminated through psychotherapy, the stimulus which becomes a cue is a repressed motive. It is when insight is gained into this motive or when it is symbolized in consciousness (perceived) that it becomes a cue to the non-rewarding consequences of the neurotic behavior. Rogers gives a clear example of the principle stated in a case involving hostile motivation:

"Mrs. L. . . . is well aware of the struggle she has had in disciplining Jim. She tells of her own hostile feelings toward him. She becomes aware, through talking of the situation, of the fact that he does many things to gain attention. Then comes the perception of these elements in a new relationship—she has helped to create his

problem by concentrating upon correction to the exclusion of bestowing any affection" (12, p. 207).

"When Mrs. L. sees clearly the satisfactions she has been obtaining through punishing her son, and the satisfactions she might gain through a more comfortable and affectionate relationship, she prefers the latter" (12, p. 209).

The 'neurotic' behavior has immediately rewarding consequences, satisfactions gained through punishing the son, and more remote non-rewarding consequences in the form of a disciplinary problem. When the stimulus to the 'neurotic' behavior, hostile feelings, becomes a cue to non-rewarding consequences of the behavior, as it does when she perceives that she has helped to create the disciplinary problem, she abandons the behavior in favor of a 'more comfortable and affectionate relationship.'

This discussion can be brought to an appropriate conclusion by considering counseling techniques best designed to make available cues which have become inaccessible through repression. These techniques, we believe, have been best described by Rogers (12). The problem which concerns us is why nondirective methods have proven effective. Roger's stress upon the time spent in counseling belonging to the client is of fundamental importance. Sargent is very explicit about this point when she states the subtle but definite distinction between 'coöperative analysis,' referred to by Sutich (16), and nondirective psychotherapy:

"The 'democratic' approach which Sutich advocates is a far step in advance of directive methods. 'Coöperative analysis,' he says, 'is based on the assumption that the client as well as the psychologist is capable of making an analytic contribution to the problem.' To admit that the client is privileged to participate in his own treatment is, however, to keep pace only half the way with Taft, Allen, and Rogers, who recognize that the counseling hour belongs wholly to the client" (13, pp. 49–50).

We believe this point is well taken since it is only through the client's having ample opportunity for verbalizing under the proper conditions, of which we shall say more shortly, that repressed cues can become available, since verbalizing is, of course, symbolizing, *i.e.*, the opposite of repression or non-symbolizing.

Rogers has also emphasized the futility of trying to speed the achievement of insight. Our analysis of repression supports this point. It has been observed that repression or non-symbolization reduces the anxiety-evoking capacity of a 'dangerous impulse.' When an act succeeds in removing a stimulus to the arousal of tension, anxiety in this instance, it reduces the tension at the same time and thereby guarantees its own reinforcement or reward. An act that has been rewarded cannot be extinguished immediately, and the more it has been rewarded, the less susceptible is it to extinction. Since repression carries its own reward and has benefited from a number of repetitions prior to counseling, it is not likely that it will be given up immediately. Therefore, neither is it likely, alas, that insight will be achieved at the outset of counseling. That repression will be relinquished only slowly is shown in bold relief in the following excerpt from an interview with Mrs. L. to whom reference has been made in a previous quotation from Rogers:

"*C.* I wonder which he feels more strongly—that you disapprove of things he does, or that underneath you really love him?

"*S.* I don't know. I don't know how he really feels. I know how I talk to him, but— Of course he hasn't said it recently, but he used to say that we didn't love him, because we corrected him. And then when he'd say that, I'd say, 'Now, listen, Jim, if

I didn't love you, I wouldn't care what you did at all. You could do just exactly what you pleased, and if I didn't love you it wouldn't make any difference to me. I wouldn't care how you turned out, but I want you to turn out to be a good man.'

"*C.* Sometimes people get a good bit of pleasure and good feeling out of just a little show of affection and love entirely separate and apart from any behavior situation. (*Pause.*)

"*S.* (*Slowly.*) I suppose I've tried so hard at trying to correct him that I haven't taken time off to— I'm not a very affectionate person by nature, not with anybody. (*Pause.*) My mother often remarked about that in regard to her. I never did go around kissing, even my mother. My brother did, and my mother used to remark that I must not love her as much as my brother did. I just didn't care anything about it.

"*C.* Do you feel sometimes that you would like to show more affection than you do show?

"*S.* (*Laughing, almost giggling.*) Oh, no" (12, pp. 177–178).

Rogers makes the following comments on this excerpt:

"One can see, as in a slow-motion camera study, the coming of new insight to this mother as she thinks out loud, 'I suppose I've worked so hard at trying to correct him that I haven't taken time off to—' Obviously the concluding portion of the idea is 'to be affectionate,' but Mrs. L. cannot face or accept the self-accusation which this implies. . . .

"During the following week, however, this dawning perception begins to grow, because she has not been made defensive in regard to it" (12, p. 178).

The latter part of the last sentence furnishes the clue to the principal condition under which verbalization overcomes repression, *i.e.*, opportunity to talk when there is no necessity for being defensive. Still another quotation from Rogers is relevant:

"By the counselor's acceptance of his statements, by the complete lack of any moralistic or judgmental attitude, by the understanding attitude which pervades the counseling interview, the client comes to recognize that all feelings and attitudes may be expressed. No attitude is too aggressive, no feeling too guilty or shameful to bring into the relationship" (12, p. 88).

When a disapproved impulse is verbalized in one way or another (hostility is verbalized to some degree in the above excerpt even though Mrs. L. 'shies off' from complete recognition of it) and is not disapproved, extinction of repression becomes possible. An experiment by Culler, Finch, Girden, and Brogden (1) enables us to analyze this extinction. Dogs were shocked on the paw whenever they failed to withdraw the leg in response to a conditioned stimulus. After the conditioned response had been established, the shock was omitted. Hilgard and Marquis describe the progress of extinction:

"Extinction can be secured only if the dog fails to respond for some reason and thereby 'discovers' that the tone is no longer followed by shock" (5, p. 60).

The animal's avoidance response can be compared to repression. When symbolization occurs for some reason, and the reason is that the client has to verbalize (this is especially true in non-directive counseling) in the course of which he is likely to symbolize disapproved impulses in one way or another, the client 'discovers' disapproval is not present, as the dogs 'discovered' that shock was not present, and extinction of repression is thereby secured.

A final word will help make clear the perspective which has guided the writing of this paper. Clinical psychology has gone through a period of trial and error and psychologists have gradually gained more precise knowledge of effective counseling procedures. Theoretical psychology has developed more or less inedependently, and many have

deplored what has appeared to them to be the theoretical psychologist's preoccupation with isolated laboratory phenomena. The analysis of repression and insight in psychotherapy presented in this paper takes its departure from animal learning experiments. While it may be a far cry from these experiments to such complex phenomena as repression and insight in psychotherapy, the principles of learning discovered in animal experiments may afford the basis for a theoretical frame of reference for clinical psychology. The practical task then would become applying principles inferred from theory which, it seems likely, would lead more rapidly to discoveries of effective techniques in counseling than a purely trial and error procedure; this is in no sense meant, we might add, to disparage the trial and error efforts that have been made nor the knowledge gained from such efforts.

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THE CONCEPT OF RIGIDITY: A CRITICAL EVALUATION

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I. Ambiguity of the Term 'Rigidity'

The term 'rigidity' has proven to be a valuable concept for the analysis of behavior in genetic, comparative and abnormal psychology. Unfortunately, there has arisen in recent literature considerable confusion about the meaning of the term resulting in contradictory interpretations of the same facts.

For instance, in accordance with many observations and experimental data, this author contended that rigidity decreases with ontogenetic as well as phylogenetic development (7). Exactly the opposite statement has been made by a student of K. Lewin, J. Kounin. He poses the hypothesis that "rigidity is a positive monotonous function of chronological age" (3). Peculiarly enough, Kounin finds himself here in disagreement with the following view of Lewin: "If our conception of a certain equivalence between higher degree of differentiation and external mobility be correct, then, under certain conditions, even with the feebleminded, a greater mobility of actual behavior should make itself noticeable with an increase in age" (4, p. 235).

These and other contradictions do not stem from the facts. As our discussion will attempt to show, they can be traced back to the ambiguity of the concept 'rigidity.'

There seem to be mainly three reasons of the ambiguity. First, various authors have made statements contradictory to one another because the concept of rigidity has been defined *structurally* by some, *functionally* by others. Second, confusion between the concepts of *rigidity* and *stability* (*differentiation*) has led to equivocal statements. Third, unwarranted generalizations have sometimes been derived from the assumption that rigidity is a *uniform* rather than a *multiform* trait.

The three reasons for the ambiguity of the rigidity concept are intimately related to one another; nevertheless, for the sake of clarity, they will be treated separately in the following discussion.

II. Functional versus Structural Concept of Rigidity

Rigidity, taken in its functional sense, refers to sluggishness in the variation of a response. Lack of variability of response has been found to be characteristic of organisms relatively low in the phylogenetic and ontogenetic scale. This trait has been observed in brain-injured organisms, animals as well as human beings. Lack of variability appears also as behavior trait of organically unimpaired, frustrated individuals.

Attempts have been made recently to relate the concept of rigidity to a *structural* organization of personality. The term has taken on a literal, *viz.* physical, meaning. Kounin in particular, employing Lewin's theory of personality structure, formulated an hypothesis of rigidity as a quasi-material property of mental organization. A person, as well as his psychological environment, is thought to be structured in the form of 'regions' with boundaries. Rigidity refers to the degree of segregation, that is, the relative independence of regions; the greater the independence the greater the rigidity.

The usefulness of this novel definition appears to be doubtful. It does not take into account sufficiently the func-

tional dynamics underlying behavior; it leads, in certain instances, to a complete reversal of common sense interpretations by describing behavior in words contrary to their generally accepted meaning.

To illustrate this we shall discuss first some experiments on satiation conducted by Kounin and by Lewin. Kounin requested his subjects—feebleminded persons of various ages, and young normal children—to draw a number of cats. After having become satiated with the drawing of cats they were asked to draw bugs; after their satiation with the drawing of bugs, they had to draw turtles, and later on, rabbits. The assumption was that the activity of drawing one type of animal co-satiates them, in varying degrees, with the drawing of other kinds of animals. Co-satiation was measured in terms of the relation between the satiation time on cats and the satiation time on bugs, etc. Co-satiation was highest with young normal children, lowest with old feebleminded subjects. Since co-satiation is assumed to be an expression of 'communication' between regions of activity, it was concluded that the group of normal children is least rigid, the group of old feebleminded persons most rigid.

Peculiarly enough, Lewin, employing a functional concept of rigidity, has drawn similar conclusions from apparently opposite results.[1] Here, the children were asked to draw moon faces over and over again. After having reached the point of satiation each child was urged to draw whatever he wished. Normal children were satiated relatively early with the drawing of moon faces but complied readily with the request to continue drawing in a free manner. Feebleminded children drew moon faces for a longer time than normal children, but the period of free drawing was very brief. ". . . the feebleminded [children]," concluded Lewin, "displayed more persistence in one and the same activity than did the normal children. Once satiated, however, they would have nothing more to do with any part of the sphere" (4, p. 200). Lewin's explanation is in terms of functional rigidity: the feebleminded child perseverates more easily with a certain task but has difficulty in shifting to another activity of the same sphere. Kounin contends that easy shift from one (satiated) activity to another of the same sphere is a sign of rigidity. Lewin contends that lack of disposition toward shifting from one satiated activity to another of the same sphere is a sign of rigidity.

Lewin's conclusions are probably sounder than Kounin's; but both authors seem to neglect the dynamic relation of a certain task to the particular mental make-up of the subject. In the light of this relationship the seeming contradiction between the results of the two experiments disappears. Any monotonous task, such as drawing moon faces or cats for a considerable time, is usually disagreeable to a normal child because it interferes with his desire for free and spontaneous activity. He welcomes the opportunity to shift to spontaneous drawing. As a rule feebleminded children are not opposed to monotonous work; they rather like it. Everybody familiar with feebleminded children has noticed the stereotypy even of their free activity. Many of these children never get tired of drawing the same objects, such as an aeroplane, or a cowboy, or a car. Because of these differences of the attitude of normal and feebleminded children toward monotonous work, definite conclusions as to structural rigidity cannot be drawn from satiation experiments. Since feebleminded children possess a considerable tolerance to monotonous work it is

[1] *vid.* K. Goldstein's critique of Kounin's experiments (2).

probably easy to persuade them to draw bugs after cats, and turtles after bugs. A feebleminded child changes easily to a similar drawing and continues with it, probably not because the two activities are rigidly set one against the other but because he welcomes small changes within an otherwise stereotyped activity. That this behavior cannot be accounted for by a low degree of co-satiation between cat-drawing and bug-drawing is shown by the fact that a greater objective difference between the drawing activities may have opposite effect. If the feebleminded child is requested to draw freely after having been satiated with stereotyped drawing he may, as Lewin has shown, continue only for a very short while. Though Kounin and Lewin's results seem contradictory, actually Kounin demonstrated only once more what Lewin had already shown, and that is: feebleminded, compared with normal persons, display a higher degree of functional rigidity, *viz.*, a greater perseverance in pursuing a monotonous task.

In terms of satiation the results of both experiments can be interpreted as follows: feebleminded persons are less easily satiated than normal individuals if the activity is monotonous or stereotyped; with spontaneous activity, normal subjects are less easily satiated than feebleminded subjects. The results of both experiments can thus be reconciled in terms of functional but not of structural rigidity.

The inadequacy of a structural concept of rigidity can be shown further by the fact that 'overlapping,' and not segregation, frequently leads to perseveration. Werner and Strauss (5, 8) performed the following experiment with two groups of feebleminded children, one with, the other without, brain injury. The subject was asked to repeat several lists of words immediately after presentation of each list. The first list of each part contained four words; these were presented and reproduced four times. The second list contained six words that were presented and reproduced only once. With non-brain-injured children, the four-word list appeared to be a closely knit unit and did not interfere with the reproduction of the six-word list. On the other hand, with brain-injured children, perseveration of one of the words of the shorter list frequently occurred during reproduction of the six-word series; this could only mean that these children organized words of the short list loosely, that is, as more or less discrete elements. Whereas complete separation of the four-word list from the six-word list hindered perseveration in the non-brain-injured group, perseveration emerged in brain-injured children through the 'overlapping' of the two word lists. Employing the structural conception of Kounin, one would be led to the absurd conclusion that brain-injured children are less rigid than normal children and non-brain-injured feebleminded subjects.

Previously we have shown that brain-injured children, just as brain-injured adults, display difficulty in shifting from one situation to another (1, 6). For instance, when working on problems in addition, it is difficult for them to shift to subtraction, or to a less related activity such as drawing. If we would attempt to employ Kounin's term of rigidity we would be forced again to this contradictory statement: compared with normal and generally feebleminded subjects, brain-injured children appear less rigid in certain experiments, such as the word list test, but more rigid in others that demand shifting.

No such contradiction exists if the functional concept of rigidity is applied. Perseverative behavior in both instances is due to disintegration or isolation. In the word list test, the

trace of a word, normally part of a comprehensive unit, becomes an isolated event which, if sufficiently strong, interferes with subsequent activity. In the shifting test, it is an attitude or an activity pattern which, divorced from the situation at hand, perseverates.

The experimental facts, just described, have a bearing on the problem concerning the relation of rigidity and state of differentiation. Some time ago we cautioned against confusing 'rigidity' with 'stability' (7). "If anything," we said then, "they are rather at odds in their psychological connotation. . . . The less differentiated . . . the more rigid and yet less stable will the behavior be. Stability of behavior requires a flexibility of response in order to preserve the functional equilibrium of the organism in the face of mutable situations."

This very confusion of the terms rigidity and stability or differentiation can be found in recent literature. The surprising statement by Kounin, ". . . rigidity is a positive function of chronological age," is obviously based on the erroneous identification of the two concepts. It is not difficult to see why a structural concept of rigidity might invite a mistake of this sort. Rigidity is here defined in terms of segregation of regions; the distinction between 'segregation of regions' and 'differentiation of regions' is easily obscured. To illustrate this point let us examine two more experiments conducted by Kounin.

The task of one experiment consisted in the insertion of marbles, one at a time. Each marble could be released, either by depressing a lever, or by raising it. During the first series of 90 trials the subject was requested to depress the lever; during the second series of 60 trials the lever had to be raised. Young normal children, more frequently than feebleminded subjects, perseverated during the second part of the experiment with the previously learned habit of depressing, instead of raising, the lever. Kounin concluded that the two situations 'overlapped' for the normal young child, but were rigidly segregated from each other by the feebleminded child. Kounin's conclusion, that feebleminded persons are more rigid then normal individuals, seems to concur with the opinion generally held by clinical psychologists. His argumentation, however, contains a complete reversal of the common, and clinically well established, concept of rigidity.

Everybody familiar with the behavior of older feebleminded children knows that their performance of simple motor tasks is hardly different from that of normal children of the same chronological age. The paradoxical conclusion which one would have to draw, if a structural concept of rigidity is to be employed, would be this: "The younger the normal child the less well is he able to shift motorically; he is more perseverative, and, therefore, less rigid." Adaptability appears here as a consequence of rigidity, instead of flexibility.

One may become aware of the clinical and genetic variety of rigid behavior, by distinguishing the following three instances:

(1) The 'regions of personality' of an *immature individual* are little differentiated; it is therefore to be expected that mutual interference, in the form of perseveration and stereotypy, should occur frequently.

(2) In a *mentally growing* organism the regions become more differentiated; a differentiated behavior emerges, varying with changes of situation (functional stability and flexibility). This differentiation does not mean separation of regions; a transfer of training, for instance, would not be possible if the regions did not intercommunicate.

(3) If regions are severed from one

another, intercommunication ceases, and a state of rigidity, due to 'isolation,' prevails. Only in this particular—and, as we shall see later, pathological—instance, does the concept of rigidity approach the structural term as used by Kounin.

The same confusion between rigidity and differentiatedness can be found in Kounin's interpretation of experiments dealing with concept formation. Cards on which figures were drawn had to be sorted according to color or to shape. Kounin reports that young feebleminded children, after having sorted the cards according to color, had less difficulty than older feebleminded subjects in sorting them according to shape. Young normal children had still less difficulty to shift than young feebleminded subjects.

The card-sorting experiment and the marble experiment have one feature in common: they both are concerned with the ability to shift. With regard to this ability the results of the two experiments seem to contradict each other. The shift on the marble test is easiest for old feebleminded persons; the shift on the card-sorting test is easiest for young normal children. It seems hardly possible to reconcile this contradiction on the basis of a structural concept of rigidity. On the motor test, feebleminded persons acted closest to normal adult behavior, whereas in the card-sorting test, the young normal children did. Therefore, logically, the results of either one of the two tests, but not of both, can prove a high degree of rigidity of feebleminded persons. The truth most probably is that the feebleminded behave more rigidly on the card-sorting test, but less rigidly on the motor test, than do young normal children. That subnormal individuals are more rigid than normal children on the card-sorting test, is probably not due to rigidly bounded and segregated regions of personality, but the result of their concrete manner of thinking. Immature persons sort by means of perceptual concrete configurations rather than by means of abstract concepts such as color or form categories. Once a concrete configuration has been formed it is much more resistant to reclassification than a grouping based on abstract concepts. Concrete thinking, as we have discussed previously (7), is a less differentiated activity than abstract thinking; therefore one can relate this rigidity directly to a signal characteristic of the subnormal personality, namely, to the lack of functional differentiation. Furthermore, it has been shown that the rigidity of shifting from one aspect to another decreases normally with chronological age. Usnadze and others (7, p. 239) have demonstrated that, on various sorting tests, young normal children display little capacity to shift; as they grow older this capacity increases. In other words, in many situations in which transfer of habits, or the restructuring of the field is required, young normal children and older feebleminded persons, as compared with more mature individuals, may behave quite similarly. If Kounin were familiar with these well-known experiments, he hardly would have made the statement that rigidity increases with age.

The discrepancy in the behavior of feebleminded, as compared with normal children, on sensori-motor and sorting tests can be explained by the different nature of the tasks. Older feebleminded persons show little retardation on simple concrete motor tasks; they perform here in a more mature manner than chronologically younger normal children. These younger children act sensori-motorically in terms of global inseparable wholes; therefore if part changes are demanded (raising instead of depressing a lever), they will either

perseverate with a learned pattern or make erroneous movements. On the other hand, feebleminded persons operate lower on tests demanding abstract concept formation than their mental ages indicate. A deliberate shift from one aspect to another required by any of the classification tests is possible only if one has a differentiated view of the objects in terms of their various (abstract) properties (color, form, etc.). Depending upon the difficulty of the classification test, younger normal children may either excel older feebleminded children in the ability to shift or may be equally rigid.

Lack of differentiation, varying with the type of child and the type of task, can therefore be considered as the basis of the rigidity. The following table is intended to clarify this complex relationship:

Nature of Task	Activity Pattern: Less Differentiated (rigid)	Activity Pattern: More Differentiated (flexible)
Concrete-motor	in normal young child	in normal older child and in feebleminded
Conceptual-abstract	in normal young child and in feebleminded	in normal older child

III. UNITARY VERSUS MULTIFORM RIGIDITY

A. Subnormal and abnormal rigidity. It has been generally assumed that rigidity as a behavior characteristic of feebleminded persons is a unitary trait. This assumption became questionable with the knowledge that types of feeblemindedness exist, differing from one another in their mental organization.

Werner and Strauss (5, 8) have shown that kinds of rigidity can be distinguished which vary in quantity and quality with organismic conditions. These authors were particularly interested in the relation between rigidity and two types of feeblemindedness, the one familial in origin, the other caused by injury to the brain. They compared two groups of children representing the two types of feeblemindedness respectively, and matched according to mental ages. Under strictly controlled experimental conditions the two groups were found to differ with respect to quantity and quality of perseverations. On the whole, the rigidity of mentally subnormal children of the brain-injured type seemed to be more like that of brain-injured children of normal I.Q. than that of feebleminded children of the familial type.

Any statement concerning the relation between feeblemindedness and rigidity should be qualified in regard to the particular type of mental deficiency. Kounin's assertion for instance that "rigidity is a monotonous function of feeblemindedness" is entirely too general. Conclusions drawn by various authors from experiments with children of unspecified forms of mental deficiency may be biassed toward one or the other type depending on the selection of the subjects. A possible illustration is found in Lewin's and Köpke's study on the resumption of interrupted actions in normal and feebleminded children (4, pp. 202 f.). Tasks such as modeling animals or assembling an automobile from paperstrips were used. After interruption the child was occupied with a task of similar nature. This latter task, if successfully finished, often had such a high substitute value that the original task was not resumed. Under these conditions, feebleminded children resumed the original task more often than normal children. These results, interesting as they are, cannot be considered unequivocal since one does not know

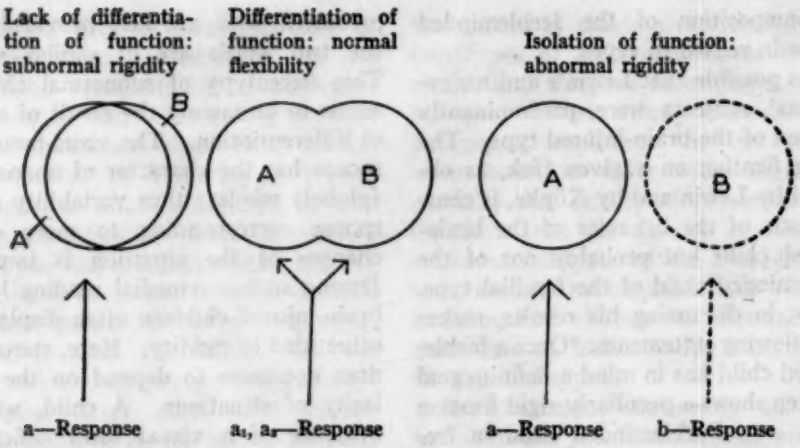
the composition of the feebleminded groups in regard to types.

It is possible that Lewin's and his coworkers' subjects were predominantly children of the brain-injured type. The strong fixation on a given task, as observed by Lewin and by Köpke, is characteristic of the behavior of the brain-injured child but probably not of the feebleminded child of the familial type. Lewin, in discussing his results, makes the following statements: "Once a feebleminded child has in mind a definite goal he often shows a peculiarly rigid fixation on it. . . . Feebleminded children frequently impress one with their pronounced stereotypy and pedantry, far exceeding the commonly observed pedantry of normal children. Thus their shoes must stand before the bed in exactly one way; the buttons on a piece of clothing must be fastened in one definite order . . . they may refuse at supper time a food generally eaten at lunch" (4, p. 204).

We hardly ever observed such pedantic behavior in school children of the familial type of feeblemindedness. These children tend toward untidiness and carelessness rather than toward pedantry. Lewin's description fits more closely the behavior of feebleminded children of the brain-injured type. Experiments and observations show these children generally to be pedantic, goal-fixated, and conspicuously clinging to certain customary habits of living.

In our own experiments (8) we have tried to set up 'critical' situations capable of separating kinds of rigidity characteristic for each of the two types of feeblemindedness. In constructing such situations we took the lead from well known everyday and clinical observations. For instance, perseverative behavior has been frequently observed in remedial reading situations. A mentally retarded beginner in reading often repeats a previously read word when presented with another, provided that the two words are of similar shape. This stereotypy of subnormal children seems to be mainly the result of a lack of differentiation. The visuo-motor response has the character of unanalyzed (global) wholes; thus variability of response corresponding to more subtle changes of the situation is impeded. During such a remedial reading lesson, brain-injured children often display another kind of rigidity. Here, stereotypy does not seem to depend on the similarity of situations. A child, without evidence of a visual form deficiency, often reiterates one particular word during presentation of a number of other words, however incongruent such a response may be ('repetitive perseveration'). Moreover, after having read correctly several words in succession, he may suddenly repeat a previously read word; and again, this response may bear no similarity to the presented word. This type of perseveration has been termed 'delayed.' Such repetitive and delayed perseverations seldom occur with feebleminded children of the familial type.

Strauss and Werner have demonstrated that it is possible to instigate these different forms of perseverations experimentally (5, 8). In one of the experiments the child had to copy a series of dot patterns, each pattern immediately after a brief exposure. Feebleminded children of the familial type perseverated with a certain pattern mainly if the one presented was of similar shape. Repetitive and delayed perseverations occurred, as a rule, only with brain-injured children. In this latter case no relationship between the child's perseverative drawing and the pattern presented was apparent. The authors concluded that one form of rigidity, found characteristically in feebleminded children of the familial type, was due to *lack of differentiation;* situ-



Three forms of responses corresponding with three forms of relationship between regions A and B.

ations which do not distinctly differ from one another tend to fuse, bringing about stereotyped responses. Another form of perseverations, *viz.* the repetitive and delayed responses, is found mainly in the brain-injured group, and is probably the result of functional *'isolation'*; a sensori-motor activity may become selfcontained to an abnormal degree, so that it may repeat itself irrespective of the incongruity of such behavior.

The following scheme is intended to represent the relationship of the two forms of rigidity with reference to normal behavior.

The diagram shows that, in relation to normal behavior, abnormal and subnormal rigidity are caused by opposite conditions. In terms of regions of personality, three forms of relationships between regions A and B may be distinguished. (1) Fusion of A and B, resulting in primitive and subnormal rigidity of young children and generally feebleminded persons; (2) Differentiation of a common sphere into two regions A and B, communicating with one another: normal variability of response a_1 and a_2, according to change of situation; (3) Separation of regions A and B: abnormal rigidity; prevalence of A leads to perseveration of a—response.

B. Primary and secondary rigidity. In a recent article Kurt Goldstein expressed the view that brain-injured patients display two kinds of rigidity called 'primary' and 'secondary' (2). Both forms are basically due to 'isolation': ". . . rigidity appears if a part of the central nervous system that is anatomically and functionally separated from the rest of the system is exposed to stimulation, . . ." (2, p. 209). Primary rigidity is independent of an impairment of higher mental processes. It is a basic lack of ability to change from one 'set' to another. This deficiency becomes apparent only if the patient attempts to shift from an activity to a task that is not related to that activity. The difficulty does not lie in the task itself; in general the patient is quite capable of solving any of these tasks even if a higher level of abstraction is demanded. The primary rigidity, or perseveration of set, has been observed especially with lesions of the subcortical apparatus. A secondary form of rigidity is the result of an impairment of abstract thinking. This rigidity appears only if the task is too

difficult; the patient, in order to avoid a complete breakdown, sticks to the task which he has solved before, repeating it over and over again. Rigidity here is a secondary phenomenon; it is a means to escape from a frustrating situation. There are other means of escape such as distractibility, for instance. Unable to master the situation the patient shifts passively from one part of the field to another. Goldstein believes that rigidity of feebleminded persons is also due to this mechanism; the mentally defective, not being able to cope with abstract tasks, becomes perseverative and distractible.

The question might be raised whether Goldstein's distinction between primary and secondary rigidity can be applied profitably to the analysis of the behavior of feebleminded children. It is doubtful that this application is feasible.

Though it is true that in feebleminded children of the brain-injured type perseverations often occur with difficult tasks, inability of shifting has been observed by us frequently independent of such difficulty.

Since feebleminded children of the brain-injured as well as of the familial type are both lacking in abstract behavior, it is difficult to see how lack of abstraction could be responsible for the quantitative and qualitative differences of rigidity mentioned previously. In some of our experiments we even noticed that brain-injured children, in comparison with the other group, perseverated more often with tasks which —judged by the amount of errors—were easier for them than for the non-brain-impaired children.

According to Goldstein, 'stimulus bond' and abnormal distractibility are behavior characteristics which accompany secondary rigidity. Distractibility and stimulus bond are, however, characteristic mainly of feeblemindedness due to brain-injury, and are seldom present in the familial type.

Delayed and repetitive perseverations typical for brain-injured, but rarely present in the non-brain-injured children, seem to point to an inner fixation that resembles Goldstein's 'primary' rigidity more than his 'secondary' rigidity. Much of the stereotyped behavior of brain-injured children could quite adequately be called 'set-rigidity' and be explained by the isolation of certain events from the continuity of experience.

Rigidity of the generally feebleminded child seems to be more closely related to a retardation in intellectual development, and therefore, to Goldstein's secondary form. However, this rigidity is not connected, as mentioned before, with the other secondary signs, such as stimulus bond and distractibility. Furthermore, it appears not only in tasks demanding abstract behavior, such as in sorting tests, but also in concrete-perceptual and motor situations which demand a discerning capacity which the immature person does not possess. I believe therefore that it is preferable to define the cause in this form of rigidity as lack of differentiation of functions rather than lack of abstract behavior. Undifferentiatedness seems to be the basic characteristic of subnormality; concreteness is only one of several signs of undifferentiated behavior.

To be sure, with both types of feebleminded children, rigidity is sometimes used as a device to avoid 'catastrophic reactions'; in this respect it is 'secondary' in Goldstein's sense. Such a teleological mechanism does not however render the two kinds of rigidity identical. There are basic differences which, as we have attempted to show, can be explained best in relation to the differences in the mental organization of the two types.

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HOWELLS ON THE HEREDITARY DIFFERENTIAL IN LEARNING—A CRITICISM

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In a recent article Howells states that "the meaning of *heredity* should be the *innate differential in development*" (2, 25). He also states that the influence of heredity, as defined above, was demonstrated in an experiment published by Vine and himself (3), in which bantam chicks were trained in a discrimination box to secure food by (1) going out on the side of the box on which a bantam chick was visible behind a glass window and avoiding a leghorn chick placed on the other side of the box and (2) going out on the side of a leghorn chick rather than a bantam. The same training was given leghorn chicks. In Howells' words: "Equal numbers of bantam and leghorn chicks were hatched together, reared together, and each chick given exactly the same training to go to his own kind of chick for food as to go to a chick of the other kind. In other words, *heredity was the only thing that could make any difference* in learning to go to a chick of own kind rather than to a chick of the other kind. *Learning to go to own kind was reliably faster and easier,* a fact which demonstrated, as claimed above, that with the same training from birth some creatures learn some behaviors easier than other creatures" (2, 25; italics mine).

The present writer does not take issue with the definition of heredity above quoted, but he believes that the evidence furnished by this experiment falls short of supporting Howells' conclusions.

In the first place, it is not logical to say that "heredity was the only thing that could make any difference" in this learning. The two kinds of chick were raised together until they reached the age of three weeks, at which time training began. In view of the extensive material published on 'despotism' among chickens (4), it seems reasonable to suppose that the larger chicks, the leghorns, would dominate the smaller ones, pecking and bullying them and interfering with their access to food. This domination would set up in the bantams an aversion to the leghorns, and this aversion would account for the fact that the bantams learned more readily to go toward one of their own kind than toward one of the leghorns. The factor of domination would not affect the learning of the leghorns in a similar situation. Whether this despotism existed would depend perhaps on the size of the living-pen and the number of food containers available as well as the difference in size and strength of the two varieties. In order to exclude the effect of such a factor, the experiment should be repeated with chicks which have been raised in complete isolation. Only then could one say that "heredity was the only thing that could make any difference."

Secondly, a close examination of Howells and Vine's data makes the hypothesis just stated more plausible, since the data are in harmony with it. Howells states in the above quotation, "Learning to go to own kind was reliably faster and easier." This statement is correct as to the bantams, but it is not correct for the leghorns; the authors expressly state that the difference found for the latter was not statistically significant. A significant difference was found for the bantams:

165.7 trials on the average for going toward own kind, 248.4 for going toward the other kind. The difference for the leghorns amounted to only ten per cent (240 and 264 trials, respectively). The value of the critical ratio for this difference is not given in the original article. The difference found for the bantams is so large that when both sets of data are put together the difference between the general averages (194 and 255, respectively) still remains significant. But to group the data in this way in an experiment of this kind and to take the averages from the grouped data to support the conclusion that "learning to go to own kind was reliably faster and easier" is to commit what Dunlap (1) calls 'the fallacy of the average animal.' If the experiment were repeated, it is quite probable that not even as much as a ten per cent difference would be found in the learning of the leghorns.

The results of the experiment by Howells and Vine can thus be explained by the hypothesis that non-hereditary factors produced the differences found. Whether this hypothesis is correct can be determined only by a repetition of the experiment in which each individual chick is raised in complete isolation from social contacts.

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IN DEFENSE OF THE THEORY OF ORDER—A REPLY TO IVAN D. LONDON *

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I. Introduction

In the history of every field of study there has been criticism and rebuttal of this criticism. It is a time-honored procedure, and, in general, has been conducted in a friendly tone. Many great names can be associated with such printed debate, among them Kant, Darwin, and Newton. Usually, it is quite beneficial for the field in question—even more so than articles having the form of sheer exposition. Such criticism and rebuttal quickly acquaints the reader with the theory, brings the dubious features of this theory to the front, and commonly assists in its refutation or substantiation.

However, the above type of publication is of value only when it has as its object the calm and impassionate clarification of the issues at stake, *i.e.*, when the interest of the debating parties is in the subject matter. Unfortunately, this is not always the case. It is a lamentable fact that scientists become no less emotional than anybody else, particularly where their own theories are concerned. Thus, many criticisms become charged with invective to a point where the critic himself appears foolish. Still others are written not to clarify or refute a given view, but to bring attention to the writer. Such criticism is not only valueless; it is actually detrimental. It confuses the issue. It defames the opposition.

Now, it is not within the province of psychologists to be as familiar with the advanced phases of physics and mathematics as they are with their own subject. Nonetheless, there are many who have such knowledge. Ivan D. London, aware of the common lack of such information in the psychological reading public, has attacked those theories which have part of their bases in physics and mathematics. The first of these attacks was directed against the topological theories of Kurt Lewin (5). To the date of this writing, Lewin has ignored this attack—so far as the author knows. In this paper we shall do the same, for it is not our purpose here to discuss the legitimacy of arguments for or against Lewin while he himself has not yet published an answer.

The second (and doubtlessly not the last) such attack was directed against Sickles' Theory of Order and the psycho-geometry supporting this theory (6). Believing that London's objective in criticizing the Theory of Order was the partial or complete refutation of the theory, an honest effort shall be made here to examine the pertinent points of difference—where these exist. However, it will not be an easy task, for these will have to be painfully extracted from the bulk of the article, which is concerned largely with confused statements of the author's position based on what appears to be misinterpretation or partial reading of his work, general and apparently unfounded accusations, and an over-all emotional exposition. Fortunately, such a task is lightened by those not infrequent passages where his statements are obviously contrary to accepted fact.

* The opinions expressed in this article are those of the author and are not to be construed as reflecting the official attitude of the Army of the United States.

The simplest manner of going about this job is to divide the criticism into two categories: first, general charges making specific accusations against the author; and second, the supporting statements from London's paper attempting to uphold the charges, plus explanatory data by the author.

II. General Charges

Let us examine the general charges against Sickles. The indictment is serious. It might be classified somewhat as follows:

1. *Ignorance of mathematics and physics and/or purposeful misuse of the terminology and concepts of these sciences.* To wit: "Sickles' contributions . . . suggest a consistent failure to observe the requisite precautions . . . strongly indicated by Sickles' usage of the concepts and terminology of physics and mathematics in a manner both foreign and contradictory to common practice there" (6, p. 199). Again: "The 'fuzziness' which inheres in Sickles' use of the language, concepts, and conventions of both physics and mathematics makes it difficult to pin any statement down with definiteness of meaning and renders debatable every step of his argument from the initial assumptions to the culminating conclusion" (6, p. 200).

2. *Fallacious theorizing on the basis of item 1, above.* To wit: ". . . the fundamental contentions of his theory are not what he makes them out to be and, hence, are invalid" (6, p. 200). Again: "The theoretical presuppositions are debatable and at variance with what is commonly held as true in both physics and mathematics" (6, p. 205).

The above cases are but a few of the charges made against the Theory of Order and its author. It would be impossible to quote them all without quoting the article itself—for the above vein of accusations runs throughout the entire criticism as one general indictment. Behind it all lies London's inherent assumption that the psychological reading public, as well as the defendant, is grossly uninformed on most of the facts of physics and mathematics. The initial thesis with which he opens this criticism, as well as that against Lewin, is that psychologists are misusing the terms of physics and mathematics, attributing false meanings to these terms, and drawing conclusions or laying down false 'laws' not consistent with the initial meanings of the terms. *I must admit that, in this general charge, London is partially correct.* But it is neither so common nor so serious as he makes it out to be—and it certainly does not call for the use of such invective as is demonstrated by the forgoing accusations. *In this paper the author will show that he is not guilty of these charges*—and the invective is therefore particularly misdirected in his case. Strangely enough, London seems less concerned with a defense of psychology than with the supposed transgressions against the terminology of the exact sciences.

III. Specific Charges

A: The determinants of lines. When one reads such statements as those of London, given above, he expects very direct evidence—evidence so exact that it proves the truth of the charges beyond all reasonable doubt. London feels he has such proof—but Sickles responds with a categorical denial on all points. However, we shall examine his evidence point by point. Every effort shall be made to state his arguments as he gives them, with the meaning intended. That this is done, the reader may assure himself by an examination of the original article (6). The initial statement shall be given, wherever possible, followed by London's criticism. We shall then examine the criticism.

Sickles: "Any given line between any two points, when devoid of all other determining lines or points, is an impossibility, a non-existent thing possessing neither qualitative nor quantitative characteristics. It can be neither straight nor curved, because it lacks determining coördinates. . . . Any line, *in vacuo*, has to be determined by at least three points in order to have any characteristics whatever" (12, p. 409).

London: "Do not two points determine a line? Does not projective geometry conceive of a line as a set of points, even if only two in number, and develop much of its theory without frames of reference? Are mathematicians concerned with the qualitative characteristics of lines? Are not any three points in general non-collinear and determine a circle sooner than a straight line? And what can a vacuum have to do with the truth of a mathematical proposition?" (6, p. 200).

Sickles: It is well that London started with the above statement, for it reveals both the superficiality and the incoherence of his criticism. Five questions, indeed—and these not too well related! What kind of criticism is this? He says on his own authority that a mathematician would not be inclined to make such a statement. I can say nothing regarding the inclinations of mathematicians. However, one need not be possessed of the *afflatus mathematicus* to recognize the statement as true. For example, a high school student could answer London's first question experimentally and judge for himself whether two points do or do not determine a line. *His answer would be in the negative.* Two points, in and of themselves, do not determine any given line—regardless of the position of the points. If the reader will take a pencil and make two dots on a sheet of paper he will immediately realize that, *through these two points may be drawn an infinity of lines*—including circles of various radius, parabolas, hyperbolas, etc., etc., without end. The only innate differences between lines are their relative extension and degree of curvature. *Between any two points may be drawn an infinity of lines of indefinite extension and curvature varying from the zero curvature of the straight line to that approximating two separate and independent parallel lines, one through each point.* Since two points do not determine any given line, then, no line is determined.

London's *second* question whether a line can be conceived as a set of points, two in number, must also be answered in the negative—for the reasons given above. But how projective geometry develops its theory is entirely irrelevant and has nothing to do with the issue. His *third* question is strictly philosophical and has to do with semantics and the definition of the word 'qualitative.' His *fourth* question concerning whether three non-collinear points do not sooner determine a circle than a straight line is, like his first question, ridiculously simple. *Again, the answer is flatly no!* The only thing we can truthfully say about three non-collinear points is that they do not determine a straight line—this by definition. But they determine a circle no sooner or better than they determine any other curve. In his *fifth* and last question London is quibbling with the rhetorical phrase 'in vacuo.' The author's meaning was clear. But, if he wishes to quibble, Webster gives the word 'void' as a synonym for vacuum—and the author meant void. However, discussion of such superficial trivialities need not occupy us here.

B: The mathematical principle of continuity and the assumptions of order. The next passage with which London takes issue is a long one and involves several important statements. In his usual manner he does not single out or attack any of these statements or the

issues involved. Rather, he condemns the passage as a whole. Let us, then, examine it.

Sickles: "Properties which can be demonstrated for a given figure (in projective geometry) are directly dependent upon the rules under which the figure was produced, *i.e.*, the initial order involved. These principles of order underlying all mathematics are also the basic laws underlying all natural processes. . . . Whenever a thing functions, the forces determining the function will also determine a configuration. This configuration . . . will correspond to the map of any other identical function irrespective of location. . . . All identical movements and functions, whatever their intrinsic nature, determine identical configurations which are subject to the geometric laws inherent in the existence of the forces involved" (12, p. 418).

London: "A mathematician and physicist would be frankly perplexed . . . because he knows that the concept of geometrical order is unnecessary for the development of much of projective geometry. . . . The physicist would be perplexed because he is getting to understand more and more, especially since the advent of quantum theory, that it is *he* who imposes mathematical order and law on natural phenomena and not nature; because he no longer regards force as an indispensable concept, explicitly dispensing with it in both relativity and quantum theory . . ." (6, p. 200).

Sickles: The five remaining lines of this criticism involve the 'sub-atomic world' of quantum theory and are wholly irrelevant. Hence, they are omitted. Though London's criticism is quite general, he does state that mathematicians and physicists would be perplexed. Therefore, it is necessary to clarify the above statements by Sickles. But it would be helpful to know just *which* scientists are perplexed—for the first of the above statements is merely a brief summary of the principle of continuity. True, the wording has been altered slightly—but the meaning is intact. This should have proposed no difficulties for a man of London's mathematical prowess. The continuity principle was probably first assumed by Kepler, then by Desargues. It may be that it was formulated by Poncelet, for a statement of it is to be found in his *Traité des propriétés projectives des figures* (1822). More recent and condensed statements may be found in Patterson (9, p. 100), Dowling (1, p. 77), and most any other text on projective geometry. Specifically, it means that the property of a geometric figure which continues to be the property of the figure, as the figure varies in a prescribed manner, will be the property of the figure in its limiting position. Thus, the property depends on the conditions according to which the figure was constructed or produced. It is admitted that this mathematical principle rests upon intuitive bases rather than rigorous logic. But we are not arguing here the validity of the principle. As a matter of fact, I cannot understand why London brought up the issue at all, if his interest was that of refuting the Theory of Order, for it has little if anything to do with the essential features of the theory.

Now, as to the statements by Sickles concerning the principles of order underlying mathematics. There are many of these assumptions of order underlying mathematics. Some are stated. But some are so simple as to be taken for granted. For instance, we assume that the unit 'one' always has the same value —and therefore the difference between 'three' and 'four' will exactly equal the difference between 'six' and 'seven.' We assume that, if the points A, B, C are in the order (A B C), and are distinct and collinear, then they are not in

the order (B C A). Some of these assumptions are to be found in Veblen and Young (14, p. 32). I have yet to find one of them that can be logically disputed. That they hold true both in mathematics and the world *per se*, is a foregone conclusion—else the mathematics could not be used to describe this world. London says, however, "that finite spaces are derivable in which neither order nor continuity exist" (6, p. 200). True enough! It is granted that the assumptions of order do not hold in certain projective spaces. But this paper is not concerned with the many 'irrational' and 'surrealist' spaces of pure mathematics. It is concerned with the space we *observe*. And *these assumptions do hold true in the class of ordered projective spaces*. "This class includes the rational and real projective spaces and many others" (14, p. 32). They hold true for the space we know in physics and psychology—but whether they would hold true in the geometry of a space of '*n* dimensions' (just as easily derived as a space of four dimensions) I do not know—and further, I cannot see how such questions bear upon the matter at hand.

Mathematics is a language of highly symbolized form. English is a language of verbal form. Both have their rules of use, and the validity of the statements of both depends on the relation of such statements to natural phenomena. In English we can describe a 'Land of Oz' and use perfect grammar in so doing. In mathematics we can derive finite but non-existent spaces and never once trespass against the rules of mathematics. But just as the happenings in 'Oz' do not pertain to natural phenomena, neither do many of the symbolized 'Ozes' of pure mathematics. It is from these that London frequently argues, and it is *he*, not Sickles, who thereby misuses the terminology and concepts of mathematics!

Who, then, is perplexed?—London? I fear it is London. He has an axe to grind, and it is an *old* axe. His preoccupation with the Theory of Indeterminism of Heisenberg (4) demonstrates that. It is a fight which began back in ancient Greece—between the 'free-willers' and the proponents of determinism. It was an 'intellectual prize-fight ring' all through the Middle Ages. And today it still survives as one of the most common problems of psychology and philosophy. His statement, given above, at one time both betrays his position and explains his criticism of Sickles—to wit: ". . . he (the physicist) is getting to understand more and more . . . that it is *he* who imposes mathematical order and law on natural phenomena and not nature" (6, p. 200). The facts of the case could not be more perverted! The physicist merely investigates natural phenomena. The results he writes down in the symbolic language of mathematics. If there is some recurrent phenomenon, he expresses it as such (usually after much rechecking of his experiments)—and behold! We have a new natural law! *If the physicist sees order in natural phenomena, then the order was there for him to see! He does not impose anything—and he would be the first to admit it!*

C: The nature of the field. To prove that the "fundamental contentions of his (Sickles') theory are not what he makes them out to be and hence, are invalid," London brings up the following arguments. He points out, and rightly enough, that the Theory of Order stands or falls on the question of a reference frame, or coördinate system. Sickles states that such a reference frame exists within the electromagnetic field—and it is with this concept that London quarrels. He does not quote the author's work, but his paraphrasing of the same, where it deals with the work in question, is correct. The state-

ments of Sickles lead to the conclusion that an electric current automatically and almost instantaneously sets up an electromagnetic field of tension—the three primary directions of which are perpendicular to each other (11, p. 193). Thus, a current produces a field at right angles with its direction of flow, and this field acts in a direction always at right angles with its diameters. Thus, we have three perpendiculars set up—the direction of current flow, the diameter or spread of the resulting field, and the direction of magnetic action of this field. These constitute three mutually perpendicular directions, and hence, a tridimensional Cartesian coördinate system—a reference frame possessing complete internal organization (12, p. 420).

London goes into elementary physics to some length explaining the structure of the electromagnetic field. Though he does not anywhere deny the above, he arrives at the conclusion that, in place of a Cartesian system of coördinates, "the electromagnetic field suggests rather cylindrical coördinates as the most natural." And again: "From Sickles' standpoint which should be considered the most natural coördinate system: the Cartesian or the cylindrical? One should think the cylindrical" (6, p. 200).

Again, London is mistaken. He says that the field is set up cylindrically (6, p. 201). Not so! It only *appears* to be set up cylindrically. If it behooves us to go into high school geometry, then be it known that a cylinder has fixed dimensions, fixed surface, and fixed volume. Whereas, on the contrary, *every electromagnetic field is of infinite diameter!* True, the strength of the field diminishes in a manner inversely proportional to the square of the distance as one recedes out this diameter from the current. But it never reaches zero. It is infinite. And theoretically, at least, even those fields set up by our telephone wires could be detected on the moon were there instruments of sufficient sensitivity and selectivity! How, then, can it be said that the field is cylindrical? To describe it analogically, the field surrounding an electric current is actually more like a small but violent vortex in space itself—surrounding which vortex the tension extends in all directions in ever diminishing degree and, theoretically, affects *all* space.

Perhaps, as London suggests, a cylindrical coördinate system could be applied. In fact, there should be an *infinity* of coördinate systems which could be applied! *But there is none that would be as simple, both in structure and in function, as the Cartesian system—and the directions of this are innate within the structure of the field itself!* Viewing the current from the direction of the field diameter, *i.e.*, perpendicular to the plane in which the current moves, it will be seen that the tension distribution is bilaterally symmetrical and, viewed from the direction of the current's movement, it is radially symmetrical. Now, the straight line (basis of the Cartesian coördinate system) is the mean of symmetry (11, p. 192). Biologists and psychologists are acutely aware of the role which is played by symmetry in the development of life. Anyone at all familiar with the process of mitosis alone, so fundamental to the existence and development of all cellular life, is indeed astonished at the vast dependence of this process upon both radial and bilateral symmetry. As a matter of fact, field forces of electrical genesis are postulated to be the logical explanation of this process (13, pp. 298–324). It is the conviction of the Theory of Order that symmetry plays an equally significant role in the psychological structure of all thinking creatures. As an integral part of the structure making such symmetry possible,

stands the Cartesian coördinate system —the frame of reference made so much of by Koffka (3), Mach (7), and others.

D: Parallelism of electrical vectors and their inter-attraction and repulsion. In his next point of difference with the Theory of Order, London does not quote Sickles—but he does present the author's viewpoint adequately. He says, "Great emphasis is placed by Sickles on the fact that two parallel wires carrying an electric current in the same direction will attract each other, from which fact he argues that parallel lines in a visual field will tend to bring order and coherence to that field because of a similar attraction" (6, p. 201).

But London goes on to say that this is analogically illusory. He states, ". . . it is easy, if one were to follow an identical argument, to show that parallelism can disrupt as well as unify a visual field" (6, p. 201). He then goes into an explanation of elementary physics to show that two parallel currents moving in opposite directions will repulse each other in a degree equal to the attraction that would be present if they moved in the same direction. He then asks, "Now, why should this contradictory case be ignored, merely because Sickles' wording of a definition on parallelism verbally suggests the other case and limits consideration to it alone" (6, pp. 202–3)?

London is probably right in saying that this case should not have been ignored. The truth of the matter is that the explanation is so obvious the author assumed the reader would recognize the existence of this situation and be able to answer it for himself. Judging from London's vehement attack, such an assumption was unwarranted. Thus, the two cases in question are diagrammed below. Figure I is the case where two parallels *attract* each other. Figure II is the case where they *repulse* each other. Let us examine Figure I first. In this figure it will be seen that there is but one negative area (that of A and A′), and similarly, only one positive area (B and B′). The potential difference between these areas causes the direction of the current to be the same for both parallel lines. Hence, they attract each other. At the same time, within the areas A A′ and B B′ we have a sameness of sign which brings about a repulsion keeping these points and the resulting parallel lines separate. On the other hand, in Figure II, the currents would move in opposite directions. Presuming a dielectric constancy of the intervening medium in which the areas are located (this presumption for purposes of explanation only), it will be immediately seen that *the area A A′ would cancel itself out of existence—as would the area B B′*. So far as electrical vectors between A B and A′ B′ are concerned, *there would be none!* In other words, London's contradictory case exists only where it is necessary to the structure of the field—and as such, is not contradictory. It does not disrupt the field. For every action there is an equal and opposite reaction—and it is this which maintains the field as such.

It is admitted that Figures I and II are very stylized diagrams. However, they illustrate the basic principle involved. The reader may draw other

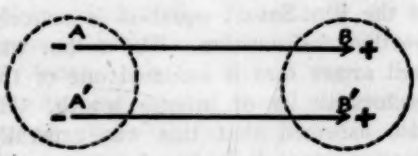

FIG. I

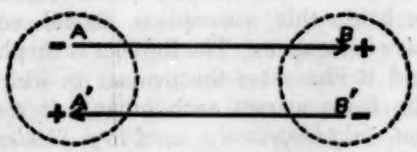

FIG. II

diagrams, more specific and less formal, for himself—and when he does so, he will find that London's so-called 'contradictory case' *occurs only where the visual field itself utilizes this property in maintaining its structural integrity.* But what about a *balanced* case—such as a square? It is very simple. In such a situation, given a constancy of the dielectric medium, negative areas should be the same distance from each other as they would be from positive areas. And what happens?—*we have a fluctuation!* First the vertical lines of a square attract our attention more than the horizontals, and then vice versa. It is not so noticeable in a plane figure like a square, however, as it is if we project this square into a cube. There we can alter the perspective almost at will! Other cases are familiar—the illusion of the reversible stairs, the chair, etc. *In each such case, simple diagramming will assure the reader for himself that an electromagnetic field, similarly structured, could be oriented with two different and equal (or nearly equal) directions of current flow—with, of course, resulting equivalent fields.* Further examination will surprise the reader. He will find that, just as he suspects, if these are *not* equal the figure has a preferred system of orientation and he has difficulty in changing the perspective from this system!

London's comment on the author's use of the Biot-Savart equation is scarcely worthy of discussion. The author was well aware that it assumed one of the vectors to be of infinite length. He also assumed that this was generally known—since it is found in most college texts on freshman physics. Again, perhaps, this assumption should not have been made. The formula is simple, and it illustrates the manner in which the fields attract each other. *It was not inappropriately used by Sickles!* But whether the Biot-Savart equation be used for illustration, or the longer formula for finite lengths, graciously given in a footnote by London (6, p. 203), it is of no consequence. The underlying geometry will remain the same. And it was this geometric field structure which the author wished to explain.

E: The structural constancy of psychological and physical space. In this section of his criticism, London rides perilously close to a mathematical 'Land of Oz.' As a result, his arguments are both irrelevant and erroneous in their implications. The question is whether the physics of Newton and Galileo and the geometry of Euclid are still applicable in the realm of everyday experience considering the recent developments in relativity and quantum theory. London says they are not—but he says so on his own authority. Sickles says they are applicable.

Sickles: "With respect to the forces which constitute the known world, a straight line may be defined by the following theorem: the path of least resistance taken by any force through any constant medium between any two points is said to be a straight line. Because space is a constant medium within the limit of human activity, the straight line is also the shortest distance between any two points" (11, pp. 190–1).

London: "But neither mathematical nor 'natural' space is to be regarded as a medium and as such constant, unless one wishes to resurrect the aether concept, long discarded, and identify it once more with space. And not straight lines, but geodesics are the natural paths of particles in the universe" (6, p. 203).

Sickles: Considering the space of the universe as a whole, its shape and structure are not constant. It is doubtlessly quasi-spherical, and its 'local' nature will depend upon the density of matter in the area in question. But the author was not aware that the Theory

of Order involved a discussion of astrophysical space or relativity—and the statement given above certainly does not. It is qualified, if London will notice, by the phrase 'within the limit of human activity'—and the author flatly denies that this necessitates a presupposition of the aether theory. As a matter of fact, *we can deal with the space of which we have direct knowledge exactly as if it were a constant medium.* It is only when we get into astrophysics and sub-atomic physics (which London is so fond of introducing) that we encounter difficulty with the concept of space as a medium. It loses its constancy. At the same time, and for the same reason, Euclidean geometry loses much of its value—for a straight line is no longer the shortest distance between two widely separated points—and the interior angles of a trangle no longer equal 180 degrees. As London points out, geodesics are then the shortest distance between two points. *However, within the limit of human activity, the space of which we have direct knowledge is such an infinitesimally small fraction of the whole that its curvature, as well as the curvature of its representative geodesics, is so minute as to be negligible.* In this vast intermediate realm, this curvature is to all intents and purposes the zero curvature of the straight line and the projection of this line into a plane! Here, Newton and Galileo cannot be assailed, and the geometry of Euclid still stands inviolate. A straight line is still the shortest distance between two points. Parallels, in accordance with the Fifth Postulate of Euclid, may still be considered as not meeting at infinity. The interior angles of a triangle still equal a straight line. And space may be dealt with as if it were a constant medium. And furthermore, *to do so does not mean that we assume the aether theory!* It merely means that we recognize the existence of a negligible error of curvature. Thompson, dealing with the same realm of matter as the author, recognized the same questions, and came to the same conclusion. He says, ". . . certain great physico-mathematical concepts have greatly changed. Newtonian mechanics and Newtonian concepts of space and time are found unsuitable, even untenable or invalid, for the all-but-infinitely great and the all-but-infinitely small. . . . But the world of the living, wide as it may be, is bounded by a familiar horizon within which our thoughts and senses are at home, our scales of time and magnitude suffice, and the Natural Philosophy of Newton and Galileo rests secure" (13, p. 20).

It is admitted, and readily, that the psycho-geometry of order—as given by Sickles—is based on Euclid and upon the physics of Newton. It is also recognized and admitted that these, insofar as they are so based, are not in full accord with relativity and quantum theory. It is denied, however, that this basis makes our resulting conclusions invalid—and we also deny that it involves the aether theory. We in the field of psychology must deal with what few quantitative tools we have available. *And if the Theory of Order involves the use of concepts and terminology of physics and mathematics "in a manner both foreign and contradictory to common practice there," for the reasons as London gives them, and if they are for these reasons invalid, then all the quantitative labor of psychology, biology, biophysics, optics, and all the branches of engineering is equally invalid—for the physics and geometry of relativity and quantum theory do not apply to these fields and cannot be used by them!* To such fields, they are a 'Land of Oz,' and it would be clumsy,

if not impossible, to survey and calculate one's farm acres by means of the non-Euclidean geometry of Lobachevsky or Riemann.

In his next paragraph, London attacks the author's statement to the effect that a straight line is the only division of an area which can result in symmetry (11, p. 191). He does not deny this statement. He cannot deny it—for it is too easy to demonstrate geometrically. He merely says that it does not hold true in such orderly but *asymmetrical* configurations as 'various spirals.' To quote him, these ". . . while not symmetrical in the strict sense of the word, most certainly feel symmetrical and possess, moreover, a high degree of that something which is called order" (6, p. 204). However, London does not explain what he means when he says spirals 'feel symmetrical.' It is a curious phrase from one who unsheathes the sword for such semantic puritanism in the sciences of mathematics and physics.

Continuing his attack on the importance of symmetry, as given in the Theory of Order, London says, ". . . the role assigned by Sickles to symmetry . . . which makes man's entire evolution dependent on such considerations, is at variance with modern knowledge, since asymmetry appears early as an integral part of vital phenomena" (6, p. 204). He then goes on to point out that chemical asymmetry is an essential phenomenon in the structure of living organisms.

That chemical asymmetry exists as a fundamental characteristic of organic life one certainly could not deny. However, London conveniently forgets to mention that *asymmetry occurs only on the molecular level. It is not common to cellular life.* Of course molecular asymmetry is necessary. As Eulenberg-Wiener states, "Owing to its asymmetric configuration the bio-molecule possesses specificity, a property peculiar to living matter, which, like the affinity of inorganic compounds, is a function of outwardly directed forces emanating from the electromagnetic fields which constitute the molecule" (2, p. 114).

But just how, may I ask, does the admitted asymmetry of bio-molecules refute the generally recognized fact that symmetry is an active determinant in the evolution of organic life? We have already spoken of its influence in mitosis—and it would be just as simple to show that the first form of organized behavior, the tropismic reaction, is dependent upon symmetry just as Jaques Loeb proved it to be. So far as the author knows, *there is not a single creature from the protozoa to the human whose vital processes do not depend, to a great measure, upon symmetry!* Let us take just one step in the process of evolution, the worm, and just one form of symmetry, bilateral. In speaking of the superiority of the structural behavior of worms over that of lower animals, Maier and Schneirla note, "Bilateral symmetry in structure and in function is primarily responsible" (8, p. 101). Considering these generally recognized facts, upon what authority does London state that symmetry as an active factor in evolution is a view 'at variance with modern knowledge'? He gives no authority for so foolish a statement. Perhaps the leading authority on the relationship between form and growth is Sir D'Arcy Thompson, who says, ". . . symmetry is highly characteristic of organic forms, and is rarely absent in living things . . ." (13, p. 357). His book (13) is to be recommended—for the larger proportion of its 1095 pages is concerned with exactly these problems.

F: The Ponzo experiment and the 'plus-sign phenomenon.' Under the title of 'Experimental Verification,' London

cites two items: the Ponzo experiment (10), and the 'plus-sign phenomenon' (11, p. 194). These, he states, are given by the author as 'actual proofs.' *Deliberate misconstruing of the facts could not be less true!* Nowhere does the author give 'actual proofs'—and he purposely avoids this implication. The Theory of Order, despite the evidence in its favor, is still unproved—just as are most theories.

The first of these items, the Ponzo experiment, London dismisses as 'inconclusive'—which no one would deny. It was cited by Sickles as evidence, *not proof*. As evidence, not conclusive proof, the author is willing to let it stand. Such was all that was intended.

Regarding the second of these items, the 'plus-sign phenomenon,' London states that the author has misconstrued 'its true nature' (6, p. 205). *He is correct!* After performing the experiment outlined by London (6, p. 205), the author is quite willing to admit that he was mistaken, and that the 'plus-sign phenomenon' is actually a result of light diffraction as London explains it to be. It is therefore with genuine gratitude that the author accepts London's explanation. In his original paper of ten and a half pages, Sickles spent a brief paragraph on the phenomenon in question (11, p. 194). It is not worth more here. Whether the phenomenon is due to field action or to diffraction, *the Theory of Order is not affected in the slightest*. It was cited to support the existence of a coördinate system. *That it fails to support this does not mean that it refutes it. It simply becomes inapplicable.* More than this would be required to refute the thesis of a coördinate system. For one thing, one would have to explain away much of the evidence gathered by Koffka (3), Mach (7), and many others to a lesser degree. As a matter of fact, the psychological use of a coördinate system can be derived from logic alone. Let us imagine two points, distinct, alone, and segregated from all other determinant data. Point *A* moves from position *A* to position *B*. What could we deduce from this sans coördinate system and arbitrary dimensional standards? We could say only that the path of movement is a line! We could not say how far it moved, what direction it moved, or whether its path was straight or curved! In short, we could ascribe to this movement no qualities whatever. Now, in observing this movement, the human actually perceives spatial qualities nonetheless. For instance, he says it moves to the left and downwards. To the left from *what?* Downwards from *what?* Has he not supplied a coordinate system? Would the judgment have been possible without it?

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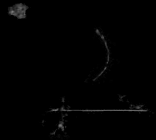
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